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THE EFFECT OF CLIMATE ON SOIL FORMATION.

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IN a recent volume[1] of the Journal of the Royal Agricultural Society of England, Russell deals with some of the effects of climate and weather on soils.

In the opening paragraphs the effect of climate on the formation of "the mineral framework of the soil" is alluded to and the laterite of the Tropics is mentioned as an example of a soil produced "under wholly different conditions" of climate, *i.e.* different from the climate of Europe.

There are two soils in India whose mode of formation is still unexplained. The laterite is one and the other is the Regur or Black Cotton Soil, the latter occupying very much the larger area. These two soils are very distinct in their several chief characteristics.

The Regur is black or dark brown in colour; when wetted it expands to an unusual degree and on drying large fissures form in it; it retains unusually large amounts of water; it is practically devoid of stones except where near rock formations; in chemical composition it frequently contains 5 to 10 per cent. of calcium carbonate, though also the proportion of this constituent sometimes falls to less than 1 per cent. It rests on various rock formations; a large part of the area rests on the Deccan trap, but it is also found on metamorphic rocks in Southern India and on the Cuddapah quartzites.

When laterite rock is first exposed it is soft and light coloured, but it rapidly hardens on exposure and after breaking down to soil is red; it frequently or generally contains gravel, including limonite, but the soil has otherwise no very special physical features. Like the Regur

[1] Vol. LXXIV. p. 1, 1913.

however it is found to pass abruptly into very various formations, trap, metamorphic, sedimentary. In fact the association is remarkably alike in the two cases.

Some years ago Holland[1] drew attention to the fact that laterite is largely not a silicate but a hydroxide of aluminium, and pointed to the difficulty of explaining its formation on chemical grounds and suggested that this might be attributed to organic life with greater probability. This suggestion has not I think been followed up.

In a recent Memoir Harrison and Ramaswamy Sivan[2] have detailed the characteristics of the material which is largely responsible for the black colour of Regur, and have shown that it is a colloidal material containing iron, aluminium and silica associated with a small proportion of organic matter and is not attacked by concentrated hydrochloric acid. The circumstances in the neighbourhood of a bed of Regur are peculiar, for at one place one can see the rock taking on the dark colour of Regur, at another not far away the same rock is weathering to red soil. It is quite possible that here too we have an instance of bacterial action.

In any case the formation of these two soils cannot be simply attributed to either weather or climate. It occurred to me therefore that the above facts might be of interest to those who are concerned with the biology of soils.

[1] *Geolog. Mag.* Vol. x. (1903), p. 59.
[2] *Memoir Dept. Agri. Ind.*, Chem. Ser. Vol. II. No. 5.

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PROBABLE ERROR IN PIG-FEEDING TRIALS.

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In an earlier issue of this *Journal*[1], Robinson and Halnan have communicated the results of a statistical analysis of three sets of pig-feeding experiments from which they conclude that "the probable error of one animal in a pig-feeding experiment is in the region of 10 per cent. of the average live-weight increase."

The whole subject of the interpretation of the results of feeding experiments has since been exhaustively reviewed by Mitchell and Grindley[2]. From the results of 17 American experiments on swine, involving 507 pigs divided into 49 lots of 5 to 23 pigs each, of initial weights ranging from 24·7 lbs. to 354·4 lbs., and feeding periods ranging from 55 to 126 days, they arrive at a figure ($11\frac{1}{2}$ per cent.) for the probable error which is substantially in agreement with the above. The range of probable errors for the separate lots in all the experiments quoted above is from $2\frac{1}{2}$ to 33 per cent. of the live-weight increase.

In an earlier discussion of the subject of the experimental error in feeding experiments Wood and Stratton[3] record an experiment with four steers of similar age, weight and past history. Their live-weight increases for a period of ten months, during which they received identical feeding and general treatment, were so closely in agreement that the probable error of any one of the four animals was only about 0·5 per cent. of their average increase. Nevertheless, when put on to a fattening ration they gave divergent results and "after three months' feeding their probable error was as usual 13 per cent. of their average live-weight increase." They conclude therefore that the experimental error cannot be appreciably reduced by such careful selection of the

[1] This *Journal*, Vol. v. p. 48.

[2] University of Illinois Agric. Exp. Sta. Bulletin No. 165 (July, 1913).

[3] This *Journal*, Vol. III. p. 417.

animals and that "the requisite precision in feeding trials can only be obtained by increase of numbers, or, if that is impossible, repetition of the experiment." This conclusion is criticised by Mitchell and Grindley as being "not based upon sound and sufficient evidence" and not in harmony with the results of their own analysis of more comprehensive data. They quote, in particular, the results of three Canadian experiments with swine, comprising 10 lots of 5 to 10 pigs each, in which the probable errors are remarkably, and, with one exception (13·5 per cent.), uniformly low, averaging only 7·4 per cent. The probable errors of the separate lots, excluding the highest, ranged from 2·5 to 10·4 per cent. "The reports of these experiments are too meagre to enable one to tell what feature or features of experimental control are responsible for this low variability."

The case of the pig is clearly one for special consideration in this connection, since its more prolific breeding qualities enable a much closer equalisation of experimental lots with regard to breed and age than is possible with cattle and sheep. This is notably of advantage as regards breed, since this factor undoubtedly contributes more than any other to the disturbing complex of "individuality." This is illustrated by Mitchell and Grindley from the records of 18 lots of pigs, each lot consisting of a single litter, ranging from 5 to 10 pigs. In only 3 lots is the probable error greater than the average ($11\frac{1}{2}$ per cent.) of all American results as quoted above.

In further illustration of this point the records of a pig-feeding experiment carried out at the Manor Farm, Garforth (Experimental Farm of the University of Leeds), in 1913 are summarised below. A more detailed account of the objects and results of the experiment has been given elsewhere[1]. Ten Large White pigs, about 8 weeks old at the outset, were subjected for 24 weeks (June 30–December 15) to identical conditions of feeding and general treatment. The pigs were drawn from two litters and for convenience were treated as two groups, each drawn from one litter only. With one exception (No. 9) all were castrated males, but owing to the unsatisfactory progress made by No. 7 it was replaced at an early stage of the experiment by a female from the same litter. The data given in the tables for No. 7 refer to this latter animal.

Special feeding pens were used in order to make it possible for each animal to be fed separately and the amount of food supplied was carefully adjusted to the appetite of the least voracious animal. In this way it

[1] Crowther and Ruston, *Journal Bd. Agric.* XXI. 789.

was ensured that each animal consumed almost exactly the same amount of food. The foodstuffs used were wheat-bran, wheat-"sharps," pea-meal and barley-meal. The pigs were weighed weekly and gave the increases shown in Table I. At the outset difficulties were experienced in getting the pigs accustomed to the separate feeding stalls and to the rations provided. For these reasons the records of the first three weeks are regarded as abnormal and excluded from the table.

TABLE I.

	Group A					Group B					Average increase for week	Prob. error of one pig expressed as per cent. of average increase
	No. 1	No. 2	No. 3	No. 4	No. 5	No. 6	No. 7	No. 8	No. 9	No. 10		
Initial Live-weight (July 21)												
	lb. 26·5	lb. 29·0	lb. 29·5	lb. 30·5	lb. 28·0	lb. 30·0	lb. (40·2)*	lb. 30·5	lb. 32·5	lb. 28·0	lb. 29·4±·2‡	% 2½‡
Increase, in lbs.												
week	3·0	3·7	2·8	4·6	4·0	3·8	—	3·0	2·6	5·1	3·6±·2‡	16‡
,,	3·4	3·.	2·5	1·4	2·5	3·0	—	3·7	3·1	1·6	2·8±·2‡	20‡
,,	4·1	6·0	5·8	7·2	4·1	3·8	—	5·7	4·2	5·0	5·1±·3‡	15‡
,,	6·5	4·5	7·5	5·6	5·4	5·9	4·8	4·9	4·5	4·0	5·4±·2	13
,,	4·9	6·5	5·5	10·6	4·5	5·6	5·0	6·4	4·5	7·0	6·0±·4	20
,,	4·3	6·2	5·0	5·0	6·5	4·6	6·0	4·9	5·1	4·0	5·2±·2	11
,,	6·9	6·3	8·0	6·7	5·4	6·1	6·0	7·4	5·4	5·9	6·4±·2	9
,,	8·4	9·5	7·6	8·2	7·5	7·7	8·9	7·8	8·0	6·4	8·0±·2	7
,,	7·1	10·7	7·5	10·4	7·4	7·0	7·4	8·9	8·2	9·5	8·4±·3	11
,,	5·5	5·7	5·1	5·9	5·8	5·3	6·7	6·0	6·0	5·3	5·7±·1	6
,,	7·5	6·0	5·8	5·7	6·1	9·7	6·0	10·6	5·3	8·5	7·1±·4	18
,,	5·0	4·5	6·5	5·3	3·4	6·0	6·5	2·3	4·8	6·2	5·1±·3	18
,,	10·3	7·5	6·1	10·7	11·5	8·0	5·5	9·5	6·8	4·0	8·0±·5	21
,,	11·9	9·5	8·6	5·8	9·4	11·8	13·2	11·5	11·0	13·2	10·6±·5	15
,,	3·1	6·5	7·2	8·0	1·9	5·3	2·3	5·2	3·5	3·3	4·6±·4	31
,,	12·1	8·9	7·3	8·2	11·0	9·0	11·3	12·2	8·8	11·1	10·0±·4	12
,,	4·5	8·2	6·8	8·7	8·8	13·0	10·2	5·8	7·8	9·4	8·3±·5	19
,,	9·1	5·5	8·0	8·9	8·1	6·8	9·0	11·2	7·1	6·9	8·1±·3	13
,,	10·0	12·4	9·2	7·4	12·9	10·5	10·9	8·8	11·7	12·6	10·6±·4	12
,,	14·0	13·2	13·3	12·2	11·5	12·0	10·6	9·8	4·1	8·8	11·0±·6	18
,,	6·5	6·6	3·8	11·5	7·5	11·0	10·0	9·3	11·0	11·7	8·9±·6	20
Final Live-weight (December 15)												
	174·5	180·8	169·3	188·7	173·0	188·3	181·5	185·2	166·0	177·5	178·5±1·7	3
Total increase in 21 weeks												
	148·0	151·8	139·8	158·2	145·0	158·3	(141·3)†	154·7	133·5	149·5	148·8±1·9‡	3·7‡

* Weight at end of 6th week (August 11). † Increase for 18 weeks (from August 11).
‡ Average of 9 pigs.

In order to facilitate comparison the data have been condensed in Table II into 3-weekly periods, for which the averages of each group are set out separately.

The results present two features of special interest in the present connection, viz. the extremely low degree of variability between the individual records and the absence of any marked tendency for the probable error (relative to increase) to fall as the feeding progressed.

TABLE II.

Weeks of experiment	Average gain in weight for 3 weeks			Probable error of one pig expressed as percentage of average gain		
	Group A	Group B	All pigs	Group A	Group B	All pigs
	lb.	lb.	lb.	%	%	%
4– 6	11·8± ·4	11·8± ·4*	11·8± ·3†	7·9	7·5*	7·5†
7– 9	17·7± ·7	15·4± ·3	16·6± ·4	8·2	3·8	7·9
10–12	23·5± ·6	22·1± ·4	22·8± ·4	5·6	3·6	5·8
13–15	16·8± ·3	19·0± ·6	17·9± ·4	4·2	6·7	7·9
16–18	23·6± ·4	23·0± ·7	23·3± ·4	3·8	7·2	5·4
19–21	24·8± ·8	27·9± ·8	26·4± ·7	6·7	6·4	8·4
22–24	30·4± ·7	30·6± ·9	30·5± ·6	5·3	6·7	5·7
Average (21 weeks)	148·6±2·1	149·0±3·7*	148·8±1·9†	3·1	4·9*	3·7†
Average for 18 weeks (excluding 4th–6th weeks)	136·8±1·8	138·0±2·6	137·3±1·5	2·9	4·2	3·4

* 4 pigs only. † 9 pigs only.

It will be noted from Table I that, with one exception towards the end of the feeding period, the probable error of one pig never exceeded 21 per cent. of the average weekly increase, whilst for 3-weekly periods (Table II) the maximum (all pigs) was only 8·4 per cent. For the whole period (9 pigs for 21 weeks or 10 pigs for 18 weeks) the probable error of one pig was only about 3½ per cent. of the average increase—as compared with the general average for pigs of 10–12 per cent. quoted by Robinson and Halnan, and by Mitchell and Grindley. If the eight male animals alone be considered the probable error is only 2·9 per cent.

The relative constancy of the probable error throughout the course of the feeding, though not without precedent, is more surprising and

difficult to interpret. It may possibly be associated with the system of feeding adopted which ensured equal consumption as contrasted with the equal *chance* of food which ordinary methods of group feeding give to each individual. This may also have contributed to the general low variability in the gains produced.

Obviously the number of animals concerned is so very small that the results cannot be regarded as in any way affecting the validity of the conclusions arrived at from the analysis of more extensive data but they may serve, in conjunction with the American data for separate litters quoted above, to suggest the desirability of a closer study of the possibilities of the pig as an instrument for the measurement of small differences in nutritive value.

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NOTE ON THE EFFECT OF CHANGES IN THE VISCOSITY OF WATER ON THE RESULTS OF MECHANICAL ANALYSES CONDUCTED AT VARYING TEMPERATURES.

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Some discordant results obtained in mechanical analysis during a spell of cold weather led the writer to enquire into the effect of temperature on the separation of the various fractions of a soil by mechanical analysis.

According to Stokes' law, the limiting velocity of a particle of diameter a falling in a liquid of viscosity η is proportional to $\frac{a^2(\rho-\sigma)}{\eta}$, ρ being the density of the solid, and σ that of the liquid.

Now in any given separation, the depth of liquid and time of subsidence are adjusted so that all particles of diameter greater than a certain value shall be left behind on decantation. If, for any reason, η varies, since v is not changed the diameter, a, of the smallest particles which are left behind on decantation will be proportional to $\sqrt{\eta}$.

The value of η is subject to considerable change when the temperature is varied. For water, Thorpe and Rodger[1] give the formula

$$\eta = \frac{\cdot 017941}{(1 + \cdot 023120t)^{1\cdot 5423}},$$

where t = temperature in degrees centigrade.

The densities, ρ and σ, are also subject to a change with the temperature, but these are of a smaller order of magnitude and may be ignored in comparison with the changes in η.

Calculating $\frac{\eta_{5^\circ}}{\eta_{15^\circ}}$ by this formula we get $\frac{\eta_{5^\circ}}{\eta_{15^\circ}} = 1\cdot 33$.

That is to say that by a fall of 10° from 15° to 5° C., the viscosity of water increases by 33 per cent. Since the diameter a is proportional to $\sqrt{\eta}$,

$$\frac{a_{15}}{a_5} = \sqrt{\frac{1\cdot 00}{1\cdot 33}} = \frac{1\cdot 00}{1\cdot 15},$$

[1] *Phil. Trans.* 1894, A, p. 1.

where a_{15} is the diameter of the smallest particles left behind on decantation at 15° C. and a_5 the diameter of the smallest particles left behind on decantation at 5° C. From this it is seen that the limiting diameter at 5° C. is 15 per cent. greater than it is at 15° C. If there is a large proportion of particles at or about this limiting diameter it will mean that considerably smaller values will be obtained for the fraction left behind on decantation if the decantation is performed at a lower temperature.

An experiment was performed in which some ignited fine sand was well mixed with an approximately equal quantity of ignited silt. Duplicate two gram lots were taken and after stirring well with water in beakers allowed to settle through 7·5 cm. for 75 seconds. The water was at 6° C. The sedimentations were repeated six times by which time no more silt remained in suspension after 75 seconds. The residual fine sand was collected, dried for several hours and weighed. The fine sand was then mixed again with the silt from which it had been separated at 6° C. and a separation was effected in exactly the same way at 11° C. The fine sand obtained at 11° C. was again dried and weighed. A further separation was made at 16° C. in exactly the same way. The results are as follows.

	Weight of fine sand, grams		
Temperature	*A*	*B*	*Mean*
6° C.	1·053	1·062	1·058
11° C.	1·123	1·114	1·118
16° C.	1·149	1·140	1·144

Putting the weight of fine sand obtained at 6° C. as 100, we have

Temperature	Amount obtained
6° C.	100
11° C.	105·7
16° C.	108·1

This shows that a change in temperature, by altering the viscosity of water, does affect the values obtained in mechanical analysis by sedimentation. It is recommended therefore that sedimentations be carried out, as far as possible, at a uniform temperature, say 12 to 14° centigrade. So far as the writer is able to find out, this point has not up to the present been emphasized.

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ON THE PROBABLE ERROR OF SAMPLING IN SOIL SURVEYS.

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The following paper records experiments made to determine the magnitude of the error involved in the sampling of a soil for survey purposes. While it is not held that the results obtained are true for all localities, they serve to indicate the order of magnitude of the various errors.

The ordinary method of sampling a soil for survey purposes is, first to select a field uniform in itself and representative of the soil type which is being examined. Borings are then taken in various parts of the field. The borings from the top soil are united to form the soil sample. Similarly those from the subsoil are united to form the subsoil sample. The number of borings recommended in text-books is from five up to ten or twelve according to the size of the field sampled.

Now the probable error affecting the analysis of a single boring is a function of two probable errors, namely, (1) the *laboratory error*, that is the error of analytical determination (which itself includes an error of sampling from the laboratory sample), and (2) the *field error*, which is the error due to the normal variation in the composition of the soil from point to point in the field.

Now if the probable error of a determination on one boring be P, the probable laboratory error be p_1, and the probable field error be p_2,

$$P = f(p_1 p_2).$$

P, p_1 and p_2 will have different values according to the soil constituent which is determined. There will thus be a different set of P, p_1 and p_2

for nitrogen, phosphoric acid, coarse sand and so on. By a well-known formula

$$P = \sqrt{p_2^2 + p_1^2},$$

provided that, as may be assumed to be the case, the errors are independent.

The object of this investigation is to obtain values of p_2, the probable field error, for various soil constituents.

There is practically no literature on the subject. The only account of previous work accessible to the writers is in a paper by J. W. Leather[1]. Leather took duplicate samples from several plots in different localities. Each sample consisted of a mixture of a dozen borings. He found that the differences between determinations on the duplicates of nitrogen, available phosphoric acid and potash respectively, varied from nothing up to more than 20 per cent. Unfortunately the data do not give any basis for determination of probable errors due to field variation[2].

There are various papers which treat of the probable error of experimental plot yields, but since the composition of the soil is only one factor in determining crop yields, it cannot be expected that these probable errors are any measure of the probable error of sampling.

Two fields were therefore investigated, one on a drift and the other on a sedentary soil. The following notes describe the two fields.

Field A, at the College Farm, Aber, near Bangor. This field has been for many years in grass. The soil is glacial drift of local origin. It is not particularly uniform in texture and appearance and for ordinary survey purposes would be reckoned too variable.

Field B, at Cellar, Aberffraw, Anglesey. This field is now in arable cultivation. The soil is derived from the Pre-Cambrian schists. There is a certain admixture of wind blown sand since the field is not far from the sea. The soil is quite uniform in texture and appearance.

In Field A, 25 samples were taken. The diagram, Fig. 1, shows the order in which the borings were made.

Each sample was kept separately in a bag. On arrival at the laboratory all the samples were analysed according to the usual methods. The following analyses were performed: Mechanical analysis, determination of hygroscopic moisture and organic matter, and the

[1] *Trans. Chem. Soc.* 1902, p. 883.

[2] Since sending in this paper, the authors have seen a paper by Pfeiffer and Blanck, *Landw. Versuchs.-Stat.* LXXVIII. Working on the nitrogen content of an experimental field, they obtained a field error of 2·5 per cent. This was, however, over a small area and the result is scarcely of service for survey purposes.

determination of the total P_2O_5. In the case of the P_2O_5 determinations it was felt that the error of analysis would be smaller if absolute determinations were made. Accordingly, the total P_2O_5 was determined by treatment with sodium peroxide.

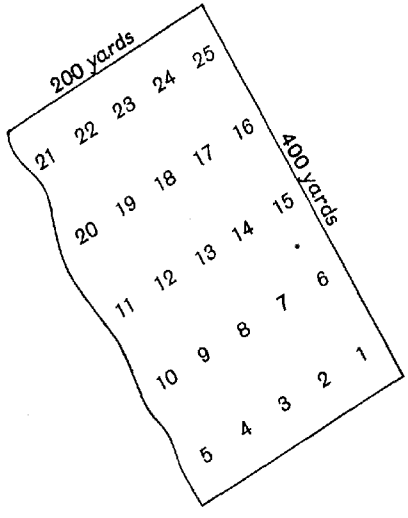


Fig. 1. Field A.

In order to get some idea of the reliability of the figures obtained, *i.e.* to determine p_1, six mechanical analyses and P_2O_5 determinations were made of a well mixed composite sample of a number of borings. The results are shown in the following table.

Composite Sample.

	Fine gravel	Coarse sand	Fine sand	Silt	Fine silt	Clay	P_2O_5
1	7·93	16·31	15·42	15·30	24·21	4·80	·343
2	7·79	18·24	15·75	14·84	23·81	5·06	·310
3	7·90	17·56	15·25	14·73	24·65	4·20	·331
4	9·67	17·78	15·53	13·65	22·80	4·88	·315
5	8·03	17·75	15·71	14·45	24·44	5·09	·323
6	8·02	18·17	14·99	14·42	24·96	4·87	·315

The mean values and probable errors calculated from these figures are as follows:

Constituent	Mean value	Probable error
Fine gravel	8·22	± ·52 = ± 6·3 per cent.
Coarse sand	17·63	± ·54 = ± 3·0 ,,
Fine sand	15·43	± ·20 = ± 1·3 ,,
Silt	14·56	± ·38 = ± 2·6 ,,
Fine silt	24·14	± ·52 = ± 2·1 ,,
Clay	4·81	± ·11 = ± 2·3 ,,
P_2O_5	·323	± ·008 = ± 2·5 ,,

Similar determinations were not made for moisture and organic matter. The determinations are made with much less trouble than the mechanical analyses and P_2O_5 estimations, and therefore the moisture and organic matter were determined four times on each boring and averaged. The agreement was so close in all cases that p_1 for these determinations would be exceedingly small both absolutely and in comparison with the corresponding values of P determined.

Fine gravel	Coarse sand	Fine sand	Silt	Fine silt	Clay	Moisture	Organic matter	Phosphorus pentoxide (P_2O_5)
2·43	14·56	18·33	13·78	27·63	4·18	3·65	12·63	·304
7·33	14·80	19·37	11·79	26·47	4·11	4·03	9·94	·261
7·84	16·02	19·20	10·37	27·99	4·06	3·24	8·55	·279
7·34	17·27	21·35	8·59	26·73	4·07	3·77	8·59	·256
7·60	15·95	16·89	10·10	29·38	5·28	3·21	8·89	·261
11·21	20·32	18·09	8·60	22·65	3·46	2·99	10·91	·365
5·32	18·71	16·60	14·95	24·30	3·66	3·62	11·33	·312
17·25	20·36	14·46	11·31	20·10	3·26	2·41	9·71	·329
12·14	19·57	14·91	12·02	21·93	3·00	2·84	12·25	·318
8·37	17·07	19·59	11·45	24·90	3·64	3·13	10·06	·281
7·42	14·59	20·87	13·56	25·12	4·47	2·46	9·61	·314
2·63	11·33	23·37	14·12	28·03	5·54	2·32	9·59	·286
12·49	22·20	14·35	11·51	21·45	3·42	2·07	9·22	·295
11·20	16·16	14·72	13·84	24·92	3·54	2·26	11·26	·298
7·72	16·20	15·67	13·41	27·34	4·35	1·92	10·30	·214
2·83	11·78	15·31	16·13	33·52	5·40	2·10	10·30	·229
8·20	17·90	21·38	12·40	24·77	4·30	1·63	10·75	·250
15·35	19·92	16·09	10·40	21·44	3·79	2·30	9·10	·198
10·56	22·52	15·52	11·39	21·25	4·01	2·63	9·65	·254
7·38	21·48	15·13	12·93	20·00	3·40	2·79	12·72	·440
11·49	20·79	15·74	10·57	22·65	4·90	2·07	9·81	·317
4·73	22·25	15·96	13·43	21·12	3·75	2·96	13·14	·435
4·00	19·25	16·76	15·28	24·25	3·58	2·68	11·69	·303
2·22	11·92	19·83	15·96	31·53	4·01	2·53	10·16	·267
3·10	16·96	19·66	13·21	26·50	6·25	2·40	8·89	·198

It will be seen from the values obtained above that with the exception of the fine gravel p_1 is about 2·5 per cent. as a maximum[1]. These figures are only obtained from six sets of determinations, but they are sufficiently accurate for the purpose of determining p_2, as will be seen later.

The table on p. 147 shows the results of the analyses of the 25 samples from Field A.

The mean values and probable errors calculated from these results are as follows:

	Per cent.	Probable error per cent. of result, P
Fine gravel	7·92 ±2·78	=±35·0 per cent.
Coarse sand	17·59 ±2·24	=±12·7 „
Fine sand	17·61 ±1·64	=± 9·3 „
Silt	12·44 ±1·24	=±10·0 „
Fine silt	26·50 ±2·71	=±10·2 „
Clay	4·10 ± ·52	=±12·7 „
Moisture	2·7 ± ·40	=±14·8 „
Organic matter	10·4 ± ·80	=± 7·7 „
Phosphorus pentoxide (P_2O_5)	·290± ·039	=±13·4 „

In Field B a set of 15 borings was taken as shown on the diagram Fig. 2.

The following results were obtained on analysis:

Aberffraw.

Fine Gravel	Coarse sand	Fine sand	Silt	Fine silt	Clay	Moisture	Organic matter	P_2O_5
4·45	43·39	18·04	8·52	12·91	3·45	2·16	5·54	·220
5·73	44·12	16·57	8·42	12·83	3·20	2·15	5·32	·209
6·13	40·79	16·23	8·81	13·95	3·15	2·42	6·10	·249
4·15	40·41	16·80	8·55	14·55	3·50	1·33	6·78	·214
7·57	42·30	16·44	8·78	14·15	3·20	1·46	5·53	·220
5·58	42·05	16·59	9·20	14·15	3·01	2·14	5·21	·204
6·05	38·97	18·37	10·06	14·86	3·64	2·09	5·22	·188
5·24	47·50	14·39	8·70	13·60	3·14	2·02	5·30	·199
3·37	46·74	16·23	9·53	12·85	3·30	2·00	5·26	·223
4·50	47·36	16·02	8·58	12·34	3·25	1·97	4·74	·264
4·95	43·56	17·11	8·98	12·86	3·82	1·94	5·23	·217
6·64	40·85	17·00	9·65	14·16	3·36	2·00	4·76	·179
4·77	43·43	16·98	8·12	13·01	3·36	2·13	5·17	·269
4·02	46·25	16·42	7·53	12·82	3·02	1·75	5·25	·229
5·40	47·46	16·56	7·35	11·49	3·17	2·06	4·99	·213

[1] In mechanical analyses this error will be much greater if the temperature of sedimentation is not constant, owing to viscosity changes.

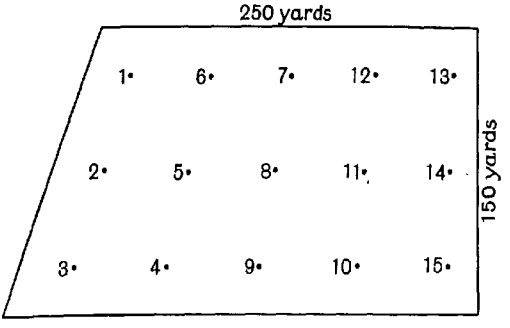


Fig. 2. Field B.

The mean values and probable errors calculated from these results are as follows:

	Per cent.	Probable error per cent. of result, P
Fine gravel	5·23 ± ·73	±13·9 per cent.
Coarse sand	43·68 ±1·76	± 4·0 ,,
Fine sand	16·65 ± ·45	± 2·7 ,,
Silt	8·73 ± ·48	± 5·5 ,,
Fine silt	13·37 ± ·60	± 4·5 ,,
Clay	3·30 ± ·13	± 3·9 ,,
Moisture	1·97 ± ·18	± 9·2 ,,
Organic matter	5·36 ± ·235	± 4·4 ,,
Phosphorus pentoxide (P_2O_5)	·220± ·017	± 7·7 ,,

The results obtained may now be stated:

Field A. Drift Soil.

Determination made	P (per cent.)	p_1 (per cent.)	$p_2 (= \sqrt{P^2 - p_1^2})$ (per cent.)
Fine gravel	35·0	6·3	34·5
Coarse sand	12·7	3·0	12·3
Fine sand	9·3	1·3	9·2
Silt	10·0	2·6	9·7
Fine silt	10·2	2·1	10·0
Clay	12·7	2·3	12·4
Moisture	14·8	v. small since mean	14·8
Organic matter	7·7	of 4 was taken	7·7
P_2O_5	13·4	2·5	13·2

Field B. Sedentary Soil.

Determination made	P (per cent.)	p_1 (per cent.)	$p_2 (= \sqrt{P^2 - p_1^2})$ (per cent.)
Fine gravel	13·9	6·3	12·4
Coarse sand	4·0	3·0	2·6
Fine sand	2·7	1·3	2·4
Silt	5·5	2·6	4·8
Fine silt	4·5	2·1	4·0
Clay	3·9	2·3	3·1
Moisture	9·2	v. small since mean of 4 was taken	9·2
Organic matter	4·4		4·4
P_2O_5	7·7	2·5	7·3

Practically the same values for p_2 might have been obtained if analyses had been repeated several times for each boring and averaged. The values of p_1 would then have been greatly reduced and consequently, as in the case of the moisture and organic matter, p_2 could be taken as approximately equal to P.

In view of the great time taken in making soil analyses it was felt that the increase in the accuracy of the values of p_2 which would thus be obtained would scarcely be great enough to justify spending four or five times as long over the laboratory work. Also, no time was spent in doing chemical analyses other than the determination of P_2O_5.

It is seen from the above tables that in the case of Field A, which is too variable for ordinary survey purposes, p_2 is as great as 34·5 per cent. for fine gravel. Now this constituent is comparatively unimportant in characterising a soil unless it predominates over other fractions. An accurate knowledge of the hygroscopic moisture is also, for survey purposes, of no great importance. Of the other determinations the probable field errors of the fractions in the mechanical analysis are about 10 per cent., while that of the P_2O_5 is 13·4 per cent.

If therefore we count on a probable error of 13·4 per cent. for this soil this can be taken as a maximum value. Similarly 7·3 per cent. can be taken as the maximum value of the field error for Field B which is a sedentary soil. Field A is as stated above of too variable a character for ordinary survey purposes. If therefore operations are conducted on the basis of a maximum field error of 10 per cent. it will probably serve for survey work.

The value of the total probable error of the final result can now be reduced to any desired value by increasing the number of borings and the number of determinations.

Thus if n borings are made and one determination made on each boring the probable error of the average is

$$\pm \frac{\sqrt{p_1^2 + p_2^2}}{\sqrt{n}} \text{ or } \frac{P}{\sqrt{n}}.$$

For example, in the case of P_2O_5 in Field A, 9 borings separately analysed and averaged would give a probable error of

$$\pm \frac{13{\cdot}4}{\sqrt{9}} = \pm 4{\cdot}46 \text{ per cent.}$$

If n borings are made and a composite sample obtained by mixing, the field error will be reduced to $\frac{p_2}{\sqrt{n}}$, but the laboratory error will still be present, so that if 9 borings from Field B were mixed, the error of one analysis would be

$$\pm \sqrt{2{\cdot}5^2 + \left(\frac{13{\cdot}2}{\sqrt{9}}\right)^2}, \text{ or } \pm 5{\cdot}05 \text{ per cent.}$$

Where the field error is large in comparison with the laboratory error not much additional accuracy is obtained by analysing the borings separately. Also, however many borings are taken and mixed, it is not possible to reduce the final error to less than the laboratory error; and it is also somewhat difficult to ensure the satisfactory mixing of a very large number of borings.

Performing more than one analysis on a composite sample will serve to reduce the laboratory error, and consequently the final error of result.

If the field error for chemical analyses be taken as ± 10 per cent. and that for the mechanical analyses as ± 5 per cent., we shall probably not err on the side of minimising errors, since no field is sampled for survey purposes unless it appears fairly uniform. As is mentioned above, Field A would not be considered uniform enough for survey purposes.

The following table shows the probable error of final result for different numbers of borings and for single and duplicate analyses of the mixed sample. The probable errors are calculated on the assumption that the final probable error is equal to the square root of the sum of the squares of the separate probable errors. Thus the probable error of an average of two determinations on a composite sample of 5 borings is in the case of mechanical analyses equal to

$$\pm \sqrt{\left(\frac{2{\cdot}5}{\sqrt{2}}\right)^2 + \left(\frac{5}{\sqrt{5}}\right)^2} = \pm 2{\cdot}8 \text{ per cent.},$$

and in the case of a chemical analysis,

$$\pm\sqrt{\left(\frac{2{\cdot}5}{\sqrt{2}}\right)^2+\left(\frac{10}{\sqrt{5}}\right)^2}=\pm 4{\cdot}8 \text{ per cent.}$$

Number of borings taken and mixed to form composite sample	Final probable error, per cent.			
	Mechanical analysis ($p_2 = \pm 5$ per cent.)		Chemical analysis ($p_2 = \pm 10$ per cent.)	
	Single analysis	Average of two analyses	Single analysis	Average of two analyses
1	±5·6	±5·3	±10·3	±10·1 %
2	±4·3	±3·95	± 7·5	± 7·3
3	±3·8	±3·5	± 6·3	± 6·0
4	±3·5	±3·05	± 5·6	± 5·3
5	±3·3	±2·8	± 5·1	± 4·8
6	±3·2	±2·7	± 4·8	± 4·4
7	±3·1	±2·6	± 4·5	± 4·2
8	±3·05	±2·5	± 4·3	± 3·95
9	±3·0	±2·4	± 4·15	± 3·75
10	±2·95	±2·35	± 4·0	± 3·6

The table shows that even with 10 borings there is not much diminution in the probable error by taking an average of two analyses. Since however some check is advisable it is better to make two analyses.

When two mechanical analyses are performed it is seen that not much additional accuracy is obtained by increasing the number of borings beyond six, either in the mechanical or chemical analyses. Taking six as the number of borings the probable error is ± 2·7 per cent. This means that it is an even chance that a result obtained is within 2·7 per cent. of the true result. The chances of a result having lower degrees of accuracy are shown in the following table, calculated from Wood and Stratton's paper on the Interpretation of Experimental results[1].

Percentage deviation from true value	Chances of result being within limits of such deviation
2·7 per cent.	Evens
3·4 ,,	3 to 2
3·9 ,,	2 to 1
4·6 ,,	3 to 1
5·1 ,,	4 to 1
5·4 ,,	9 to 2

[1] This *Journal*, Vol. III. p. 429.

Similarly in the case of a chemical analysis, we have for six borings:

Percentage deviation from true value	Chances of result being within limits of such deviation
4·4 per cent.	Evens
5·5 ,,	3 to 2
6·3 ,,	2 to 1
7·5 ,,	3 to 1
8·3 ,,	4 to 1
8·8 ,,	9 to 2

From these results it is seen that an accuracy of 5 per cent. in mechanical analysis is ensured a probability of 4 to 1 by doing a duplicate analysis on six borings. For survey purposes this is probably sufficient, since it is not conceivable that variation in the amount of any fraction corresponding to 5 per cent. (relative to the amount of the fraction) could have any effect on the properties of a soil.

In the case of chemical analysis it does not seem that the same accuracy can be expected. For survey purposes however the accuracy is probably sufficient. In the case of a critical study of one soil however it would be necessary to reduce the errors much more by repeating analyses and increasing the number of borings.

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THE STARCH EQUIVALENT THEORY.

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The account given by Wood and Yule of their investigation of the records of British Feeding Trials and the Starch Equivalent Theory in part 2 vol. VI of this *Journal* is a valuable and timely paper. It contains abundant material for reflection. Consideration of the data suggests that there is room for doubt in regard to the conclusions for which a claim to "certainty" has been advanced. It is not, however, the immediate purpose of this article to discuss these conclusions—though incidentally certain points in the investigation come under review—but rather to discern, as far as possible, the direction in which the methods and arguments employed lead, and more particularly what is their bearing on the starch equivalent theory as a practical system.

In their opening remarks Wood and Yule state that "starch equivalent for this purpose (maintenance) is reckoned by the formula: starch equivalent = digestible protein × 1·25 + digestible fat × 2·3 + amides × 0·6 + digestible carbohydrates + digestible fibre. The generally accepted maintenance ration for a 1000 lb. ox on this basis is 6·35 lb. of starch equivalent which includes 0·6 lb. of digestible protein." The author is not concerned to deny these statements but he desires to emphasise the fact that this formula is not Kellner's formula and that this estimate of the animal's requirements is not Kellner's estimate.

The formula differs from that of Kellner[1] in two respects. In the latter the protein is multiplied by 0·94 instead of 1·25, and a further correction which varies according to the estimated percentage value for fattening of each food is also made. The formula given by Wood and Yule is, as stated, intended to determine the value of the foods

[1] *The Scientific Feeding of Animals.* p. 355

for maintenance. Hence these differences. But, it may be asked, is it seriously proposed that the starch equivalent for maintenance should be reckoned by one formula and that for fattening by another? If so, the starch equivalent system will scarcely survive the shock, for it would be equally necessary to have another formula for milk production and still another for work. Kellner's system is fundamentally a mere convention. It does not establish a true "quantitative relationship between the amount of food and the amount of fat, work or milk it may be expected to produce." Its general applicability to these problems rests essentially upon uniformity of the method, and if this is undermined its more important advantages are destroyed.

The factor 1·25 is presumably based on the relative amounts of heat evolved from protein and starch when these substances are oxidised in the animal's body, whereas the factor 0·94 is based upon the relative amounts of fat formed from them. This point appears to have been appreciated by Kellner and to have been allowed for by him as shown below.

It is generally agreed that the total thermic energy of the digestible matter of the food is available for maintenance. It is, apparently, for this reason that Wood and Yule make no correction for the "value" of the food. Such correction is, however, the very essence of Kellner's system, and if it be not made the numbers should not be called "starch equivalents." The formula given by Wood and Yule is a reversion to the system of Wolff and his colleagues and predecessors. It differs from what was formerly called "total digestible nutrients" only in respect of the factor (1·25 instead of 1·0) applied to the protein; and so far as the foods commonly used for maintenance rations are concerned this difference is insignificant. The author suggests that if such reversion is to be countenanced it would be well to revert to the old-fashioned or some other name. Endless confusion must result if the term "starch equivalent" be promiscuously applied to essentially different things.

This at once becomes evident when we turn to consideration of the requirements of the animals. The specious similarity between Kellner's[1] estimate of 6 lb. and that of Wood and Yule of 6·35 lb. of starch equivalent per day for maintenance of a 1000 lb. ox is deceptive. The latter, according to the formula given, includes the whole of the thermic energy of the digestible matter of the food; the former corresponds to the dynamic portion only. In order to make this quite plain Kellner

[1] *The Scientific Feeding of Animals*, p. 392.

shows in his appendix tables[1] that the maintenance ration from which his 6 lb. of starch equivalent is derived must contain 0·6 to 0·8 lb. of digestible protein, 0·1 of digestible fat and 7·5 to 9·5 lb. of digestible carbohydrates and fibre. This amounts to from $8\frac{1}{2}$ to $10\frac{3}{4}$ lb. of "starch equivalent" reckoned by the formula given by Wood and Yule.

We have here to deal with a serious defect in Kellner's system. He found[2] that, on the average, only some 50 to 70 per cent. of the available energy of the foods used for maintenance rations is of dynamic value. Consequently the starch equivalents of these foods are from 30 to 50 per cent. below the full value. The maintenance requirements of the animals, when expressed in terms of starch equivalent, must therefore be reduced by a similar amount. The quantities of feeding stuffs necessary to maintain the animals may be estimated with approximate accuracy on this basis provided the foods are of the value assumed in the estimate of the animal's requirements, but not otherwise. For example, the quantity of maize meal which corresponds to 6 lb. of starch equivalent would be too small for maintenance of a 1000 lb. ox because it yields little or no thermic energy in addition to that represented by the starch equivalent. In practice, of course, no one would use maize meal for maintenance rations of oxen; but in the investigation undertaken by Wood and Yule the fattening ration (most of which it may be assumed was of nearly full value) was assigned for maintenance functions in order that the fattening value of the roots might be determined. In this case 6 lb. of Kellner's starch equivalent, derived from such foods, would be inadequate. As Wood and Yule did not employ Kellner's formula but another which includes the whole of the available energy of the food, this argument does not apply. It remains to be seen, however, whether the 6·35 lb. of starch equivalent, reckoned by that formula, was sufficient.

It will be seen that Kellner's estimate of 6 lb. of starch equivalent for maintenance is a purely artificial number arbitrarily adapted to the needs of his convention. That being so, it makes little difference whether the protein be multiplied by 1·25 or by 0·94. The net result of using the higher factor is that the estimates of the requirements for maintenance must be correspondingly increased. Kellner appears to have deliberately decided to ignore the difference between the heat producing and fat producing values of protein in order to attain that uniformity which his system demands, and because he saw that it

[1] *The Scientific Feeding of Animals*, p. 392.

[2] *Ibid.*, p. 360 *et seq.*

could easily be allowed for in his empirical estimate of the animal's requirements for maintenance.

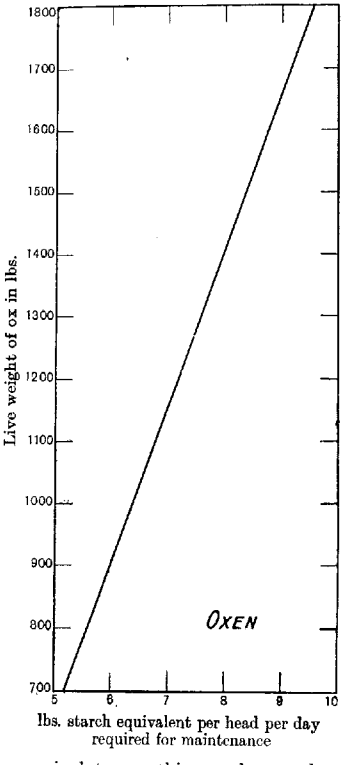
Six pounds of Kellner's starch equivalent corresponds to about 20 lb. of hay of average quality. This estimate of the requirements for maintenance of a 1000 lb. ox does not differ materially from that of Wolff[1] and the older authorities, and the author has hitherto regarded it as "the generally accepted ration." It is, however, very different from that of Wood and Yule, which corresponds to only about 13 lb. of such hay. This discrepancy cannot be lightly passed over. It is too considerable, and it is of fundamental importance in regard to this and other investigations. If they adhere to their statement it would be interesting to know upon what data it is founded. It is to be hoped that they will take an early opportunity of clearing up the matter.

It is of course possible that Wood and Yule did not intend to propose a standard radically different from that of Kellner and Wolff. The omission from their formula of all reference to the "value" of the food may be merely a typographical error, but the statement that an ox of 1000 lb. live weight when on maintenance diet evolves 10,800 Cal. of heat per day seems to preclude this explanation. It is evident that this statement is not based upon a direct determination in the calorimeter, but that 10,800 Cal. is merely the calculated equivalent of 6·35 lb. of pure starch. It may be inferred therefore, that the formula was intended to include the whole of the available energy of the food and not the dynamic portion only. The only obvious way of escape from this conclusion is to assume that the statement involves a repetition of Kellner's mistake in calculating the amount of heat an animal should evolve from the starch equivalent instead of from the total available energy of the food. He says[2] that "an ox weighing 600 Kg. requires a daily supply of 12,780 Cal."; but as this corresponds to about 10,620 Cal. for an ox of 1000 lb. it is clearly inconsistent with the ration prescribed in the tables. The latter, quoted above, represents from 14,500 to 18,350 Cal. per day of available energy, and this accords well with the estimate on the next page: "For maintenance of the animal determined from other experiments—17,320 Cal."

It has hitherto been customary to assume that the requirements for maintenance of oxen were proportional to the live weights of the animals and could therefore be determined by rule of three, though it has long been known that this is not true. A notable advance has

[1] *Farm Foods* (p. 350), Gurney and Jackson, 1895.
[2] *The Scientific Feeding of Animals*, p. 48.

been made by Wood and Yule in discarding this assumption. They determined the variation in the requirements of oxen of different sizes by means of a diagram, which by their kind permission is reproduced below



Reduced to numerical terms, this graph may be expressed by the formula:

$$\log E = \tfrac{3}{4} \log M - 1{\cdot}19723,$$

M is the live weight (lb.) of the animal and E is the amount (lb.) of starch equivalent required for maintenance.

The concordance between the graph and the formula is shown below:

M	800	1000	1200	1400	1600	1800
E (graph)	5·5	6·35	7·2	8·0	8·8	9·4
E (formula)	5·47	6·35	7·17	7·95	8·69	9·4

The figures from which the graph was constructed were taken from Kellner's Ernährung. They are not the results of direct determinations of food requirements, but, if the author is not mistaken, are derived from measurements of the relation between the body surface and live weight of certain dogs; and they involve the assumption that the requirements for maintenance vary directly as the extent of body surface. It is well that this should be recognised, but there is good reason to anticipate that when it is put to the test of experiment the assumption will prove to be well founded. Over a year ago the author published a formula[1] for maintenance rations of oxen in terms of "total digestible nutrients with an albuminoid ratio of 10 – 1." Assuming that 9·3 lb. of such nutrients is the maintenance ration for oxen of 1000 lb. live weight, this formula may be expressed as follows:

$$\log N = \tfrac{2}{3} \log M - 1{\cdot}03152.$$

This view is confirmed by independent data of a purely mathematical kind. It also derives a certain amount of support from the fact that the requirements of animals of 40 to 170 lb. live weight, calculated by this formula, are consistent with experimental data relating to the maintenance rations of pigs of that size[2]. The argument cannot be applied to the formula in terms of starch equivalent because the foods that are suitable for maintenance of pigs are of nearly "full value," *i.e.* they contain no thermic energy over and above the dynamic portion represented by the starch equivalent. It might be supposed that it could be applied in the case of other ruminants such as sheep, which can subsist on much the same kind of diet as oxen. Such, however, is not the case, because, owing to their thick coats of wool, these animals do not lose heat by radiation so rapidly as pigs and oxen in proportion to their body surface.

Wood and Yule have assessed the maintenance ration for sheep of 100 lb. live weight at about 6½ lb. of starch equivalent per week; but, on the ground that this shows a higher percentage utilisation of the food for fattening, they suspect that the allowance is too small. If the starch equivalent referred to was calculated by the formula given by them, *i.e.* if it includes the whole of the available energy of the food, there can be little doubt that it is too small. On the other hand, if it means Kellner's starch equivalent, it is probably too large.

[1] *The Chemistry of Cattle Feeding and Dairying* (Longmans), pp. 128 and 142.

[2] Sanborn's *Expers.* Bul. 28, Mo. Agr. Col. Cf. *Chemistry of Cattle Feeding and Dairying*, p. 206.

The author's estimate[1] of the maintenance requirements of sheep of 100 lb. live weight is from $7\frac{1}{2}$ to $8\frac{1}{2}$ lb. per week of "total digestible nutrients" corresponding to about 5 or $5\frac{1}{2}$ lb. of Kellner's starch equivalent. Even so, there is reason to believe that sheep retain in their bodies a larger percentage of the food in excess of the maintenance ration than oxen do.

The whole question stands in urgent need of re-examination on a fundamental basis. The estimate that the fattening increase comprises "67 per cent. of dry matter, chiefly fat, and 33 per cent. of water" is probably true only under conditions similar to those of the Rothamsted experiments in which the animals[2] appear to have been not fully grown. Comparison of the figures[3] relating to the fat ox and the half-fat ox shows that of a fattening increase of 200 lb. something like 190 lb. was fat and only 10 lb., *i.e.* about 5 per cent., was water. It may ultimately prove that the true fattening increase—apart from growth—consists almost entirely of pure fat.

It is to be noticed also in this connection that in the attempts to estimate the maintenance rations of oxen of different size by means of graphs or formulae it is assumed that all the animals are in store condition, *i.e.* that the difference in live weight is due in all cases to greater or less growth and not to fattening. Fat animals require more food to maintain them without gain or loss of weight than those in lean condition. It is not easy to see how a single graph could be adapted to meet this difficulty, but the formula given above might be modified in some such manner as that shown below:

$$\log N = \tfrac{2}{3} \log M - 1{\cdot}03152x,$$

where x is a number that varies inversely as the fatness of the animal. If any means can be found to express the degree of fatness in numerical terms it would probably not be very difficult to determine x. All that can be said at present is that when the animals are in store condition—whatever that may be—the value of x is 1, and that when they are fatter it is less than 1.

It may also be found eventually that herein lies the explanation of the gradually diminishing returns in the shape of increase for food consumed as the animal becomes fatter. To say that it is due to the fact that a fat animal requires more food for maintenance is not merely

[1] *Chemistry of Cattle Feeding and Dairying* (Longmans), pp. 130 and 204.

[2] Cf. *Scientific Feeding of Animals*, pp. 254 and 255.

[3] *Rothamsted Memoirs*, Vol. III. pp. 520 and 558. Cf. *Chemistry of Cattle Feeding*, pp. 189 and 190.

a repetition of the statement in different terms, because, if true, it would be equally applicable to an animal producing milk.

The starch equivalent system is still on trial, and it is tolerably evident that it is far from perfect. It is complex in definition and difficult to apply. Farmers as a rule will have none of it, and it frequently proves a stumbling-block to scientific students. It may even betray experts as it has on occasion betrayed Kellner himself. The system originated in a praiseworthy attempt to overcome some of the more obvious objections to the old-fashioned "feeding standards" in which it was assumed that the nutrients in all kinds of foods were of equal value for all kinds of purposes. At least it is in that direction that it has found its chief application. The foregoing discussion, however, tends to show that the day of such feeding standards is over. Attempts to calculate a ration comprising two or more independent variables, *e.g.* maintenance and fattening, by a single arithmetical operation—the rule of three—which it now appears is not applicable to either are no longer defensible. Conversion of the nutrients into starch equivalents does not overcome this difficulty.

Animals require food for maintenance and for the several forms of production—growth, work, fattening, and lactation—though it rarely happens in practice that more than two, or at most three, out of these five conditions have to be satisfied at the same time in any given case. The best results will, therefore, be obtained when the digestible nitrogenous and non-nitrogenous nutrients are supplied in the proportions and quantities required for each specific purpose. The amounts required for maintenance depend upon the size of the animal and those for other purposes upon the rate of each kind of production, though probably in no case are they directly proportional to that rate. In each case the nutrients must be derived from a food suitable for the purpose. For example, the nutrients for maintenance of oxen should be derived from cheap coarse fodders, and those for fattening from the finest, most readily digestible materials. Nothing should be deducted from the former for the work of digestion, etc. From the latter there is nothing to deduct on this account. At least, except in one or two instances, the amount to be deducted is insignificant. If, however, the amounts of nutrients for the several purposes are not to be added together but directly translated into the corresponding amounts of appropriate kinds of food, it seems clear that they must be determined by a separate calculation in each case, and the *raison d'être* of the starch equivalent system disappears.

Elsewhere[1] the author has proposed that the feeding standards should be superseded by formulae. These should be in terms of "total digestible nutrients" with given albuminoid ratios for maintenance, growth, work, fattening and milk production. This would afford a simple and satisfactory solution of difficulties hitherto encountered, and it possesses many collateral advantages. Perhaps not the least of these is the fact that attempts to evolve such formulae would effectually expose our ignorance of many points at present concealed by the feeding standards and further obscured by the complexities of the starch equivalent system. In the present state of our knowledge indeed such formulae could be little more than hypotheses but they would serve to give point and direction to research.

Wood and Yule's paper shows that some progress has already been made in this direction and it encourages the hope that in the near future the rate of advance may be greatly accelerated.

[1] *Chemistry of Cattle Feeding and Dairying* (Longmans).

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THE MAINTENANCE RATION OF OXEN AND THE STARCH EQUIVALENT THEORY.

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Within recent years, Kellner's system of starch equivalents has received much criticism from various quarters, and its value as a practical feeding system has been called into question. That the system is far from perfect in its working is universally admitted, and the recent work of W. Bruce[1] with oxen has done much to emphasise this fact. In a series of practical feeding trials with oxen, Bruce obtained results contrary to those expected from the theoretical consideration of the Kellner system of starch equivalents, and largely on account of this work, and partly on account of the demonstration of the importance of the part played by certain unknown chemical compounds in the normal metabolism of animals, many workers have been led to suggest a total abolition of the Kellner system. It is, however, inconceivable to the author that a system based on 30 years continuous research upon feeding problems, should be condemned without the fullest possible enquiry. Furthermore it is supported by many experimental results. In a critical comparison of the values of certain foods for milk production theoretically computed by the Kellner system, Crowther[2] has shown that the results obtained with one exception agree very well with those obtained in actual feeding practice. Moreover, with regard to the food deficiency factors which play such an important role in the normal metabolism of animals, Hopkins[3] has shown that those factors are hardly likely to be missing in normal

[1] Bruce, "Cattle Feeding Experiments," *Edinburgh and E. of Scot. Coll. of Ag. Report*, 27.

[2] Crowther, *Science Progress*, **7**, p. 436

[3] Hopkins, *Journ. of Phys.* 1912, **44**, p. 425.

dietaries, and consequently the results obtained in ordinary feeding trials will rarely be affected by lack of these factors.

Two papers of importance on the subject of starch equivalents have recently appeared, that of Wood and Yule[1], in which an attempt has been made to explain the discrepancies existing between the German figures and the results obtained in English feeding practice; and the more recent paper of Murray[2], which not only favours total abolition of the Kellner system, but also criticises the accuracy of the results stated by Wood and Yule. The present paper has, therefore, been written with a view to clearing up the points dealt with by Murray in his paper, and criticism of Murray's views has consequently been confined to those points which bear directly on the subject under discussion.

In the first portion of the paper, Wood and Yule are criticised for suggesting the necessity of distinguishing between "starch equivalent for maintenance" and "starch equivalent for production." Recognising the difference between the thermic and dynamic values of foods, and the fact that within reasonable limits the total digestible thermic energy of a food is available for maintenance, Wood and Yule give two different formulae to be applied according as the food is intended for maintenance purposes or fattening purposes. In doing so they thus render easy a distinction between the thermic and dynamic value of a food, and one of Murray's most serious objections to the Kellner system disappears. It may be as well here to define clearly what Kellner intended by his system. Kellner aimed at giving a measure of the available energy of the excess of any ration over the maintenance ration by measuring the amount of fat produced compared with the amount of fat produced by an equal quantity of starch. The available energy once determined and stated as starch equivalent may be readily calculated to work production, milk production, or any other purpose. The starch equivalents so determined were intended to be used solely for estimating the relative fattening capacities of various foods, and were not intended to be applied indiscriminately for the estimation of maintenance and also for the production of fat. The endeavour to do so naturally led to much confusion of thought as to the exact meaning of "starch equivalent," and the attempt to interpret maintenance requirement in terms of "starch equivalent," *i.e.* "starch equivalent for fat production," caused even Kellner to confuse the

[1] Wood and Yule, *Journ. of Agric. Sci.* **6**, p. 233.
[2] Murray, *ibid.* **7**, p. 154.

issue. Under these circumstances, it is difficult to understand Murray's objection to Wood and Yule's use of "maintenance starch equivalent." especially since Murray himself admits that "much confusion of thought must result if the term 'starch equivalent' be promiscuously applied to essentially different things" and also clearly shows the impossibility of completely defining starch equivalent for maintenance by starch equivalent for production alone.

The term "starch equivalent" in the sense used by Kellner is a scientifically incomplete term, since it does not express in itself all it is intended to express, but may cover two essentially different things. Its application, therefore, is contrary to scientific usage. Since too, Kellner meant by "starch equivalent" "starch equivalent for production," why not call it so? Whether the "production starch equivalent" is determined directly, or whether it is calculated from digestible nutrients and "value" number, seems to be beside the point, and immaterial to the discussion. Once it is realised that Kellner's use of "starch equivalent" means "starch equivalent for production," the most serious defect complained of in Kellner's system disappears, since there will exist no longer any danger of the student or scientific worker using the "starch equivalent for production" of a food to express the "maintenance starch equivalent."

That the distinction between the requirements for maintenance and the requirements for fat production when expressed as "starch equivalent" and "value" number in both cases is a rather subtle one, is shown by the fact that Murray, while pointing out the fact that Kellner himself erred, failed to realize at which end of the scale Kellner erred, and thereby unknowingly dropped into the same trap that Kellner prepared for himself. It is unfortunate that Murray's authorities which he quotes should have favoured his point of view, since a consideration of the original data on which they were based would have shown him clearly that they were incorrect, and that Wood and Yule's estimate of 6·35 lbs. starch equivalent for maintenance per 1000 lbs. live weight of animal is a correct one. In other words, when Kellner[1] expressed the energy requirements of oxen at rest in terms of "starch equivalent," he found that the maintenance requirements of the ox were satisfied when 6 lbs. starch per 1000 lbs. live weight were given, and thus the "specious similarity" with Wood and Yule's figures is explained. Later on when he came to collect his data in the Appendix he evidently assumed the starch equivalent so

[1] Kellner, *Landw. Versuchs. Stat.* **53**, p. 12.

obtained to be the dynamic portion only of the food, and, reckoning the fodder as hay, allowed more thermic energy for maintenance than was required. A critical examination of the original data upon which the figures were based shows this.

Daily requirement of metabolisable energy for an ox of 632 kg. @ 15·2° C. (average of experiments)	= 13,470 cals.[1]
Calculated for a 1000 lbs. animal	= 10,840 cals.
On the assumption that 1 gr. starch yields 3·76 cals. available for maintenance of the animal body	= 6·35 lbs. M.S.E.
On the digestibility figures given by Kellner for medium meadow hay (100 lbs. hay = 43·5 lbs. maintenance starch equivalent)	= 14·5 lbs. hay.

containing 12·4 lbs. dry matter,
·67 lb. dig. crude protein,
·08 lb. dig. crude fat,
and 5·3 lbs. dig. N-free extract.

From the above-mentioned maintenance requirement of 10,840 cals. Kellner gives 6 lbs. S.E. in his appendix. Calculated as above this would represent 13·8 lbs. hay. Calculated from starch equivalent assuming it to be production starch equivalent (100 lbs. hay = 23·7 lbs. P.S.E. and 43·5 lbs. M.S.E.) this would represent 25 lbs. hay containing:

21 lbs. dry matter,
·8 lb. dig. protein,
·15 lb. dig. fat,
and 9·1 lb. dig. N-free extract.

The appendix gives 15–21 lbs. dry matter, ·6 to ·8 protein, ·1 fat, and 7·5 to 9·5 lbs. N-free extract. The error Kellner committed was therefore the reverse of what Murray assumed.

The earliest investigations with regard to maintenance rations of oxen at rest were due to Henneberg and Stohmann[2] who carried out a series of investigations at Weende Experiment Station on two mature draft oxen, weighing 516 and 575 kg. respectively. These animals were fed on various diets containing two or more of the following foods: oat straw, mangels, rape cake, clover hay and rye straw, and sufficient food was given to maintain the animals without change of body weight. In each experimental period, which varied in length from ten days to two months, a collection of faeces and urine was made during the last three weeks of each experiment, and an analysis was conducted on the excreta produced during the last three days in order to arrive at some idea of the digestibility of the foods

[1] Kellner, *Landw. Versuchs. Stat.* **53**, p. 13.

[2] Henneberg and Stohmann, Wolff's *Farm Foods*, p. 229; Henneberg and Stohmann, *Beiträge zur Begrundung einer rationellen Fütterung der Wiederkäuer*, Part I. pp. 17–188.

fed. As a result of these experiments Henneberg and Stohmann came to the conclusion that the requirements of adult oxen of 1000 lbs. live weight were satisfied with a ration containing ·57 lb. digestible protein and 7·4 lbs. digestible N-free extract, *i.e.* an available energy intake of about 13,830 cals. per day. It should be remembered that these experiments lasted for short periods only, and no respiration calorimetry control was used to check the accuracy of the figures obtained by merely weighing the animals. Moreover, in these experiments, no estimation of the fat of the foods was determined except in the case of the rape cake. The figures given, therefore, must be accepted with a certain amount of reserve.

Henneberg[1] records two instances, (*a*) at Weende, and (*b*) elsewhere, of full-grown oxen fed throughout the winter months under conditions of ordinary agricultural practice upon the following diets:

In lbs. per 1000 lbs. live weight

(*a*)	(*b*)
12·9 straw	16·3 barley straw
7·1 sainfoin hay	0·4 aftermath
0·4 bean meal	2·0 clover hay
0·4 rape cake	1·3 pea straw
	2·9 mixed barley and oat meal
containing	containing
1 lb. digestible protein	·7 lb. digestible protein
7·8 lbs. nitrogen-free extract	8·8 lbs. nitrogen-free extract

The animals fed on ration (*a*) increased 70 to 90 lbs. apiece; those on ration (*b*) performed light work without losing condition. That is to say, a diet containing ·7 lb. digestible protein and 8·8 nitrogen-free extract per 1000 lbs. live weight is more than sufficient to maintain the animal, and Henneberg's and Stohmann's estimate, and in consequence Wolff's estimate, are considerably in excess of the actual maintenance requirements of oxen. Wolff[2], as a result of critical examination of the Weende experiments, comes to the conclusion that as these experiments were conducted at a temperature of 62° to 69° F. the allowance made by Henneberg and Stohmann is rather too small, and he suggests therefore replacing it by a ration containing ·7 lb. digestible protein and 8·4 lbs. digestible nitrogen-free extract per 1000 lbs. live weight. This is the figure given by Wolff in the appendix and quoted by Murray in support of his arguments.

[1] Henneberg and Stohmann, *Beiträge zur Begrundung einer rationellen Fütterung der Wiederkäuer.*

[2] Wolff, *Farm Foods*, pp. 229 and 350.

We have already seen that this estimate of Henneberg and Stohmann and Wolff is unsupported by the practical examples given. Considerations of later calorimetry experiments support the conclusion that the above estimate is much in excess of the normal maintenance requirements of oxen. Kellner[1] fed an ox of 748 kg. on 9 kg. average meadow hay per day for 15 days. During this period the animal lost about 1 lb. weight a day, so the diet was evidently just under maintenance. For a 1000 lbs. ox this diet works out at 12 lbs. hay a day (about). The estimate of 6·35 lbs. starch equivalent for maintenance of Wood and Yule corresponds to 14 lbs. meadow hay, which agrees well with this experiment. An isolated experiment, however, is a dangerous basis for conclusions, and this becomes more evident when the figure quoted by Murray of 17,320 cals.[2] as the energy requirement for maintenance of an ox of 638·1 kg. is considered. This figure is derived from an experiment conducted by Kellner on an "ox B," whose energy requirements for maintenance were extraordinarily high owing to its uneasiness and general restlessness in the calorimeter and while under experiment. So unsatisfactory a subject did this ox prove for experimental purposes that Kellner[3] himself omitted it when making the average on which Wood and Yule's graph is based.

A notable advance in the state of our knowledge with regard to the maintenance requirements of oxen was made when Kühn[4], in a series of painstaking and careful experiments with seven oxen extending over a period of seven years, came to the conclusion that the maintenance requirements of grown oxen are satisfied with a ration containing ·7 lb. digestible protein and 6·6 lbs. digestible nitrogen-free extract per 1000 lbs. live weight per day.

In these experiments, a coarse fodder formed the sole source of the available energy (meadow or clover hay and oat straw) and the experiments included both determinations of digestibility and the total metabolism of nitrogen and carbon including the excretion of methane.

The death of Kühn left the investigation in the hands of Kellner[5], who, as a result of experiments with two oxen A and B and combining some of Kühn's earlier results, came to the conclusion that an ox of

[1] Kellner, *Landw. Versuchs. Stat.* 1898, **50**, p. 245.
[2] Goodwin, *Scientific Feeding of Animals* (Kellner's), p. 50.
[3] Kellner, *Landw. Versuchs. Stat.* 1896, **47**, 275; **50**, 273. footnote.
[4] Kühn, *Landw. Versuchs. Stat.* 1894, **44**, 257–581.
[5] Kellner, *Landw. Versuchs. Stat.* 1896, **47**, 275.

630·3 kg. live weight at 15·5° C. requires 15,167·5 cals. per day for maintenance, *i.e.* a 1000 kg. beast requires 24,000 cals. per 24 hours. This figure was obtained by averaging Kühn's oxen (II, V, VI, and XX) with ox A, ox B not being included for reasons already given above. Calculating this average on the basis of Rubner's surface law, the figure per 1000 lbs. live weight = 12,200 cals.

Further experiments with three oxen previously fed on a fattening ration led Kellner[1] to conclude that an 800 kg. animal in a fattening condition required rather more for maintenance per day than an animal of similar weight in store condition, namely 24,900 cals. per 1000 kg. per day. The advent of Rubner's surface law, and consideration of the fact that the energy calculated from an addition of flesh or fat represented roughly only 60 per cent. of the metabolisable energy of the food digested, caused Kellner[2] to recalculate on this basis the maintenance requirements of an ox at rest. From the results thus obtained Kellner constructed the following table:

Maintenance Ration.

Live weight	Calories	Digestible organic substance Meadow hay.
Observed. Average of experiments:		
632 kg.	13,470	3·85 kg.
Calculated from surface law:		
450 kg.	10,740	3·07 kg.
500	11,520	3·29
550	12,280	3·51
600	13,010	3·72
650	13,720	3·92
700	14,420	4·12
750	15,100	4·31
800	15,760	4·50

This represented an energy requirement of about 10,840 cals. per day per 1000 lb. ox, *i.e.* a maintenance starch equivalent of 6·35 lbs. a day. It is this figure that Wood and Yule read off their graph, and it is from this table that Kellner calculated the figures from which Wood and Yule's graph is derived. This estimate is, however, inconsistent with the figure given in the appendix[3] to Goodwin's translation, since the ration there suggested represents an energy requirement of

[1] Kellner, *Landw. Versuchs. Stat.* 1898, **50**, 245.
[2] Kellner, *Landw. Versuchs. Stat.* 1900, **53**, 1–474.
[3] Kellner, *Scientific Feeding of Animals*, p. 392.

14,500 to 18,350 cals. per 1000 lb. live weight per day, and is practically identical with that given in the appendix to Wolff's *Farm Foods*. We must, therefore, assume either that Kellner copied the figures straight from Wolff's table, or that he made the mistake of reckoning the amount of hay required from the starch equivalent for production instead of from the total metabolisable energy of the food. In the latter case, the mistake, the existence of which Murray recognises, is the exact reverse of that which Murray assumes, and Murray's estimate of the energy requirements of oxen at rest is much too high.

Independently of Kellner's later experiments, a long series of digestibility experiments was conducted by Armsby[1] in America on cattle. These experiments, which were of long duration, and in which the animals were weighed very frequently, support in their findings the results given by the more exact calorimetry experiments of Kellner. Kellner's experiments are open to the objection that the results given are based on an assumption in most cases of the thing Kellner was endeavouring to find, *i.e.* an exact knowledge of the maintenance requirements of an ox at rest. Where the animals used for the estimation of the average had put on weight (and five out of the seven animals used for the average had increased in weight during the experiment), Kellner added a certain percentage of energy to the total stored; since he had found in previous experiments that of 100 parts available energy only 43 parts in the case of meadow hay, and 37·6 parts in the case of oat straw, were available for fat production. But these figures were arrived at by assuming the maintenance requirements of an ox at rest to be 21,300 cals. per 1000 kilos. live weight. In other words Kellner was guilty here of the vicious practice of arguing in a circle. The difference is not large, and does not affect Kellner's estimate to a large extent, but it is essential to realize here that Kellner's estimate of the maintenance requirements of an ox at rest is not an exact one. As a result of Armsby's investigations into the maintenance requirements of cattle, the following estimate was arrived at, and represents the average of 12 different experiments. The food fed was coarse timothy hay, and the energy of the digestible food was estimated by means of the Berthelot bomb calorimeter. As a result of these determinations Armsby came to the conclusion that the energy requirements of a 500 kg. ox are satisfied with a total digestible energy of 12,771 cals. at 51° F., or 11,980 cals. per 1000 lbs. live weight.

[1] Armsby, *Bulletin* 42, Penn. State Coll. Expt. Stat. 1898.

From Kühn and Kellner's results, and the experiments of Armsby, it is evident that the estimate given by Wolff and the older authorities, and included by Kellner in his appendix for the maintenance of a 1000 lb. ox at rest, is much too high, and that the energy requirements of an ox of this weight are satisfied on a diet supplying ·6 lb. digestible protein and 10,840 cals. per day, *i.e.* a maintenance starch equivalent of 6·35 lbs. per day. In other words the "generally accepted ration" of 20 lbs. hay per day for a 1000 lb. ox is much too generous for maintenance purposes and should be replaced by a ration of 14 lbs. hay per day. Murray's criticism of the value to be attached to the conclusions reached by Wood and Yule in their paper consequently becomes pointless. The following table summarises the estimates of various authors of the maintenance requirements of an ox at rest, the results being calculated in every case to the needs of a 1000 lb. ox.

Maintenance Requirements of Oxen.

Author	Maintenance starch equivalent	In calories per 1000 lbs. L.W.
Henneberg and Stohmann	8·1	13,830
Wolff (Appendix)	9·2	15,700
Kellner (Appendix)	9·6	16,380
Kühn	7·5	12,850
Kellner (1896)	7·1	12,200
Armsby (1898)	7·0	11,980
Kellner (1900)	6·35	10,840

With regard to the graph, the data derived from *Die Ernährung* on which the graph is based certainly depend upon the application of Rubner's[1] surface law, and the data given were calculated by Kellner[2] on this basis. But the figure from which they were calculated is an average of experiments of long duration on seven oxen, and this estimate of the energy requirements of an ox, namely 13,470 cals. for maintenance of a 632 kg. ox, at 15·5° C. or 10,840 per 1000 lbs. live weight, is an experimental determination. The truth of Rubner's surface law, derived from experiments on dogs (presumably adult) varying from 3·2 to 31 kilos., is freely accepted by Murray. While in no way criticising the accuracy of Rubner's results, or the universal applicability of this law, the possibility of a modification of our ideas on this subject in the light of future research must clearly be borne in mind. Rubner's law states that the energy metabolism is proportional to the superficial area of the animal. In other words, the metabolism varies as the amount of heat loss at the surface, and its variation in accordance

[1] Rubner, *Zeitschrift für Biologie*, 1883, **19**, 535.
[2] Kellner, *Die Ernährung Land. Nutztiere*, p. 395.

with this law is necessary for the maintenance of a constant temperature. When Rubner first established this "law of surface area" he explained its constancy on the assumption that the loss of heat from a body must depend upon the extent of the surface, and stimuli received from that surface determined the amount of metabolism and so maintained the body temperature while allowing for loss of heat by radiation. But at a temperature of 30° C. where all possibility of thermal stimuli was removed, Rubner found in the case of two guinea-pigs of different sizes that the law still held, and the explanation that the variation in metabolism of different animals was due to the "chemical regulation" brought about by specific sensory influences of cold proceeding from a definite area of surface, broke down. The insufficiency of the explanation was realised by Rubner who, however, stated that even at this temperature the law is still a necessity if the general mechanism for loss of heat in the various animals is the same in all. From a physiological standpoint, it is difficult to accept this explanation. The metabolism of a body must depend upon the mass of living cells it contains, and the temperature of this mass, and the mechanism for the regulation of heat loss and the maintenance of a constant temperature, must therefore be a subsidiary one, and not the determining factor of metabolism.

Each living cell must possess a basic requirement of energy at a given temperature for its needs, and the heat produced as a result of its activity must be a secondary result. The chemical regulation consequently is only required when the heat so produced is insufficient to provide for loss by radiation, etc., and maintenance of the cell at its normal temperature. That this is so is supported by the fact that where the food given is more than sufficient to supply the energy requirements of the body, chemical regulation is not brought into play until the temperature of the surroundings has fallen to the point where the energy requirements supplied by the food are insufficient to satisfy the body's requirements. Moreover, the path of heat loss varies considerably according to the temperature of the environment. Thus, whereas in a dog at low temperature the path of the heat loss is through radiation by the skin, at a temperature of 37° C. the path is by means of evaporation of the water from the lungs, and is here independent of the surface.

The normal energy metabolism of an organism as measured by the heat loss consequently covers two things, (1) the basic energy requirement of the living cell for the adequate expression of its activity,

which requirement would be independent of the body surface and only a function of the temperature of the cell, and (2) the energy lost through difference of temperature of the animal and its surroundings, and depending obviously on the extent and nature of the surface. Where the temperature difference between the animal and its surroundings is such that all loss due to the second influence is cut out, we should expect the energy requirement to be proportional to the body weight rather than to the surface.

The data upon which Rubner's law was established are based on calorimetry experiments on dogs whose weights varied from 3·2 to 31·2 kg. Leaving out the two dogs of smallest weight the results obtained could be expressed equally well as a straight line. In other words, between 10 and 30 kilos., the differences between the energy requirement expressed as a linear function of the body weight and the energy requirement expressed as a linear function of the body surface are within the limits of experimental error. This is shown much more clearly when we deal with the data for oxen. As expressed in terms of starch equivalent for maintenance, the values calculated for oxen of different weight by means of Rubner's surface law form such a flat curve between the weights taken, that Wood and Yule found it sufficiently accurate to express these results by a straight line. Therefore, for oxen between the weights taken the maintenance requirements are, within the limits of experimental error, a linear function of the body weight, and may be expressed by the simple graph given. Murray, in his paper, proposes to express this perfectly simple relation between body weight and energy requirement by the somewhat complicated formula

$$E = \tfrac{2}{3} \log M - 1{\cdot}19723$$

where M = live weight of animal and E = maintenance starch equivalent.

This formula is obviously not based upon the graph given but upon consideration of Rubner's law of surface area, and the close relation between the results calculated from Murray's formula and the figures taken from the graph emphasize the sufficiency of the simple straight line graph of Wood and Yule for the expression of the maintenance requirements of oxen between the weights shown. As calculated by Murray, the concordance is as follows:

M	800 lb.	1000 lb.	1200 lb.	1400 lb.	1600 lb.	1800 lb.
E (graph)	5·5	6·35	7·2	8·0	8·8	9·4
E (formula)	5·47	6·35	7·17	7·95	8·69	9·4

Emphasis has been laid on this last point, since it is difficult to see how a formula such as Murray gives is going to appeal to a farmer who has already rejected Kellner's starch equivalent system on account of its complexity of application. The formula may be more scientifically accurate, and Murray should be given the credit he deserves in evolving a formula which satisfies alike the maintenance requirements of two such widely different animals as a young growing pig of 50 lbs. weight and an adult ox of 1800 lbs.; but at the same time it is felt that with regard to oxen the simpler mathematical relation given by Wood and Yule is more within the region of practical politics, and its use is thereby justified. There is much that is valuable and stimulating in Murray's paper, and the fresh aspect he brings to bear on the science of animal nutrition gives one much food for thought. Much of the criticism he brings to bear on the starch equivalent system, however, loses greatly in value when the paper of Wood and Yule is taken into consideration, since these authors show how many of the faults adherent to the Kellner system may be rectified. The beauty of the Kellner system lies in the fact that it gives the comparative values, and not the absolute values, of feeding stuffs for fattening purposes, and much careful thought should be devoted to consideration of its values and defects before deciding to substitute for a simple quantitative number mathematical formulae of varying complexity, which, though they may be scientifically more accurate, are not likely to arouse much enthusiasm among the farming community. The farmer desires to know, not how much fat or milk or work a food will produce, but rather which of several foods is more economical for any purpose he has in view. And it has yet to be proved that the starch equivalent system is incapable of giving him the right interpretation with regard to this point.

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VARIATION IN THE MALE HOP, *HUMULUS LUPULUS* L.

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The hop plant *Humulus lupulus* L. is best known in Britain from its cultivated forms grown for use in the brewing industry. Those varieties which are grown on a commercial scale in this country all conform to the general description of *Humulus lupulus* L. and the presumption is that they are all variations (directly, or indirectly through other varieties) from the female form of the original wild hop. Since the plant is dioecious it follows that any one of these varieties was in all probability derived primarily from *one* ♀ *plant* which arose either as a mutation or as a hybrid and has subsequently been propagated vegetatively by cuttings or "sets," *i.e.* the variety is represented by a number of plants comprising a *clone*[1]; such a variety is not necessarily provided with a corresponding male form possessing all the vegetative characters of that particular ♀ variety.

The varieties of the female hop are, at the present day, very numerous but no attempt has hitherto been made to distinguish varieties of the male plant, for, as Prof. Percival points out[2], "on account of their being of no use to the grower, males have never been subject to special selection and improvement."

The value of the male hop to the practical hop-grower in this country was, until a few years ago, a subject of much debate. Recent observations and experiments carried out at Wye College by Mr E. S. Salmon

[1] The word "clone" was first used by Webber in 1903 and has been adopted by Dr Johs. Schmidt, Director of the Carlsberg Laboratory, Copenhagen. In his recent paper "On the Aroma of Hops" (*Comptes-rendus des travaux du Laboratoire de Carlsberg*, 11me Vol. 1915) Dr Schmidt, in discussing the use of the word "clone," writes (*loc. cit.* footnote, p. 153): "I would suggest that the word be adopted into the terminology relating to hops, where such a term is actually needed. A hop-clone would thus be all those plants derived from the same seedling by vegetative propagation, a clone-plant being any single plant belonging to the clone."

[2] *Agricultural Botany*, 4th Ed. 1910, p. 345.

and Mr A. Amos have proved conclusively, however, that for the production of "well grown-out" hops of the best English varieties the development of a certain number of seeds is of primary importance[1]. This involves pollination, for there is no definite proof that parthenogenesis[2] obtains in the hop. Hop-growers therefore are now strongly advised to plant a certain proportion of male hops in their hop-gardens, and that this advice is being followed is shown by the numerous applications, made by farmers of Kent, Surrey, and Sussex, for the male-hop "cuttings" supplied free by Wye College[3].

In order to meet the demand for cuttings some 200 "hills" of male hops are now established in the nursery attached to the hop-garden on the College farm. An examination of the plants shows that they vary considerably in some of their characters, but of these the only ones taken into consideration as being useful to the commercial grower were (1) *Time of flowering, i.e.* early, mid-season, or late, since a male hop to be of any value for pollination purposes must necessarily be in flower with mature pollen at the time that the female plant has receptive stigmas[4]. (2) *Vigour*, including the suitability for growth on certain soils; a male hop that thrives in East Kent may not be successful when grown on the heavy soil of the Weald.

Breeding operations now being carried out by Mr Salmon for the purpose of raising new varieties suggested the advisability of obtaining descriptions, as complete as possible, of those plants used in the "crossings," particularly of the males since there are no specified varieties of that form. It was proposed therefore that the writer should study and keep under observation for a number of consecutive seasons a large number of male hops, in order to determine those characters, if any, which could be taken as a basis for a classification (not necessarily a *natural* one) and identification of varieties of male hops.

Of the hills available 120 were selected as being suitable for detailed observation. Among those chosen fourteen plants[5] were each repre-

[1] *The Journal of the South-Eastern Agric. College*, No. 17, pp. 364–391.

[2] I.e. *Somatic Parthenogenesis* of Winkler or *Parthenapogamy* of Prof. Farmer.

[3] *The Journal of the South-Eastern Agric. College*, No. 21, p. 425.

[4] When the stigmas of the ♀ flowers are receptive they project from between the bracts and bracteoles of the strobiloid inflorescence which develops into the "hop" of commerce, and the plant is "in burr."

[5] Owing to an attack of "nettle-head" disease only eleven of these were fully available for the season 1914, since two of the three hills in one case and one hill each in two others had to be "grubbed" during the winter 1913–14.

sented by three hills (*i.e.* two of the hills were obtained by planting cuttings taken from the third, so that all three are of the same clone in those cases where the original hill is known to be a seedling), and seven by two hills each, similarly obtained. The remainder are not known to have any direct vegetative connection with each other in their own generation, and it may be that each is the sole representative in the garden of a seedling plant; they include, however, four raised from cuttings obtained from *Oregon* which are indistinguishable from one another. The number of individual seedlings represented in the hills selected is therefore reduced to about 80, including 53 which have been raised as seedlings in the College nursery. The advantage derived from having two or three hills of one plant in different parts of the garden is, that the limits of variation in those characters which are influenced by environmental factors can be determined with a greater degree of accuracy.

Prof. J. Percival in his *Agricultural Botany* writes[1]: "It is somewhat curious that, although female seedlings show considerable variation, we have never seen any morphological differences among males, no matter what their origin, except in one or two solitary instances where the 'bines' were a paler colour than usual." Reference has already been made to these words by Mr Salmon, who says[2]: "This statement is somewhat misleading, since we find in the forms, or varieties, of the male hop quite as much variation in such characters as the colour of the stem and petioles, length of the lateral branches, and in other vegetative characters as in the female hop-plant." The object of the present paper is to present a detailed account of observations which warrant this conclusion.

The hills examined (120 in number and representing, as noted above, about 80 plants) showed variation in the characters set out below. The investigations have extended over three seasons, so that in some cases (*i.e.* where one individual plant has been grown in three hills) it has been possible to make nine observations of any one particular character. The system of training the hops adopted in the nursery where all these males are grown is that known as the "Butcher System[3]."

[1] *Loc. cit.* 4th Edit. 1910, p. 346.

[2] *Journal of Genetics*, Vol. III. No. 3, Feb. 1914. Footnote on p. 195.

[3] A description of this system of training hops is to be found in an article on "Hop Cultivation" by Mr A. Amos in the *Journal of the Board of Agriculture*, Feb. 1910, Vol. XVI. with figures on p. 891.

The male hops examined were found to vary in the following particulars:

Time of flowering.
Stem (bine).
Colour: green, red, or intermediate.
Ridges: rough or smooth; darker or lighter than the general colour of the stem.
Length of internodes.
Leaves. Total length of petiole and lamina.
Lamina: colour, dark or light green; flat or margins of lobes recurved towards lower surface; wrinkling of the leaf feeble or well-pronounced; number and shape of lobes; glands on lower surface numerous or few; size of glands.
Petiole: colour of upper and lower sides; roughness of lower (dorsal) surface; depth of furrow on upper (ventral) surface.
Laterals (inflorescences).
Length: absolute length and relative to subtending bract.
Number of nodes at which bracteoles are leafy.
Length of internodes.
Length of secondary laterals.
Stipules: upright, spreading or recurved.
Flower.
Perianth segments: dimensions of segments and number of glands on outer surface.
Anthers: number of glands in the outer (dorsal) furrow.
Disc: number of glands present.

In order to secure uniformity in preparing a record of observations the following plan was adopted:

The *Time of Flowering* is understood to be the first date when open flowers were seen; this was selected rather than the date when the plant was in full flower as being more easily and accurately determined. The remaining characters were taken when the plant was well in flower.

The colour of the stem was determined for that portion of the bine from the ground to the "breast-wire[1]," and the roughness of the ridges taken for a short length (about 2 feet) just above that wire.

For observations of the leaf-glands two leaves were taken from each hill, one at the level of the breast-wire, the other midway between that and the top wire.

The measurements of the laterals were taken at two levels, the first at one-third the distance from the breast-wire to the top wire, and the other at two-thirds that distance; the general observations on the leaves[2] and the length of the internodes of the bines were taken between those two levels.

[1] The "breast-wire" or middle wire of the Butcher System is about 4 feet 6 inches from the ground.

[2] Or rather *bracts*, since the laterals (inflorescences) grow in their axils.

Time of Flowering.

The records taken were the dates on which flowers were first seen open. Usually a hill will be in full flower a week or 10 days later and may remain in flower for still a fortnight longer. The period during which a plant remains in flower is modified by atmospheric conditions; a warm dry air favours the dehiscence of the anthers, while cold, moist air is unfavourable. The time of flowering for any individual varies with the age of the plant, and probably to some extent with the season (the dry summer of 1914 hastened the average time of flowering of the mature plants, though the difference was practically negligible).

The older males in the garden, *i.e.* the well-established seedlings and the plants raised by cuttings from them, represent 87 hills, and it was found that in each of three of these the date of flowering had been the same for the years 1912 and 1913; 44 had flowered earlier and 40 later during the second year, with an average date of but 0·29 of a day earlier, which seems to indicate that the seasonal variation in the flowering period for the two years was practically nil. In 1914 the average time of flowering for these mature plants was 1·6 days earlier than in 1912. Thus it will be seen that, though certain individuals may show considerable variation from one year to another, the mean time of flowering throughout the garden was, for the well-established plants at any rate, about the same for the three years, and the seasonal modifications are probably slight except during abnormal seasons.

One of our most promising early ♂ hops is one labelled Z 12 [= 292 and 293][1], which in nine observations has commenced to flower on dates ranging from June 29th to July 8th with a mean date July 3rd. Another early one is 294 [= 295] with time of flowering June 30th to July 4th with a mean date July 2nd.

Late males include E 16 [= 44 and 48], which in nine observations came into flower from July 23rd to August 6th, with July 28th as the mean date. A 13 [= 315 and 316] may be mentioned as an example as a midseason male hop; its date of flowering is July 7th to 17th with July 12th as the mean.

These observations are evidence that, just as there are early, mid-season, and late varieties of female hops, so also the same variation may be taken as a characteristic varietal feature of the male hops.

[1] The numbers in brackets refer to hills obtained by planting cuttings from the hill bearing the number in front of the bracket.

Stem (Bine).

Colour. Although the colour of the bine varies from a pale green to a dark red among the plants under observation, the variation in colour in the individual plant is very slight, in fact, among the English forms of male hops this character appears to be more constant than any other, persisting from year to year in the same hill and in different parts of the garden for hills having the same origin. This is not only true for the pale green and the dark red stems but also for the intermediate forms; in the latter the colour is not generally a uniform intermediate shade between green and red, but consists of a mottling of small areas alternately red and green. This mottled appearance is often very marked and may persist to some extent in the green bines and the red bines. In the palest green and darkest reds the mottling is not visible but when these extremes are departed from small red dots in the one case and small green dots in the other appear. In others again the dots are more conspicuous and there is either a green ground-tint with red markings or a red ground with green markings and the general appearance approaches more nearly the intermediate mottled form.

The coloration of the petiole is associated with that of the stem and usually an observation of the petiole of a leaf removed from a plant is sufficient to determine approximately the colour of the stem from which it was taken. This association of colour will be treated more fully when dealing with the petiole.

Ridges. The hop-bine is provided normally with six ridges[1] corresponding in position with the leaf-trace bundles of the vascular cylinder. In colour the ridges are almost invariably darker than the surface of the stem between the ridges. The pale green bines have ridges of a rather less pale green and the red bines have ridges of a darker red except in the very dark ones where the tendency is towards an almost uniform deep red. Occasionally the ridges may be seen to be paler than the general surface, but in such cases transitions to dark ridges will almost certainly be found on the same bine. No case has been observed where the ridges are *consistently* paler than the rest of the surface and only one plant was noticed in which they are *usually* so; this plant, J 36, is a seedling (raised from the German variety "Stirn," the male parent being *probably* Z 12) producing a mottled bine with

[1] Occasionally a bine with nine ridges and bearing a whorl of three leaves at each node is met with.

ridges generally green (but sometimes with transitions to red) and this characteristic has appeared in each of the past three seasons. Sometimes the darker colour of the ridges is very pronounced and the bine in consequence is conspicuously striped.

The ridges are beset with small wart-like emergences each bearing a T-shaped spine at its apex, the cross-piece of the T being parallel with the longitudinal axis of the stem and acutely pointed at each extremity. When these "hairs" are numerous the bine is harsh to the touch, when they are few in number it may feel almost smooth, but no plant has been met with where they are absent altogether. Whether the degrees of roughness can be accepted as characters to be used in defining varieties is not yet certain, since individuals are often by no means constant in this respect. Some plants, however, are appreciably rougher, others smoother than the average and retain the character from year to year.

The length of the internodes is probably influenced too much by the weather, method of training, general vigour and age of the plant to be of any value systematically, unless indeed vigour itself is to be looked upon as an inherited character[1]. The internodal measurements were recorded by taking the minimum and maximum lengths occurring in the middle third of the distance between the breast-wire and top wire. The internodes may vary from 8 to 13 inches even in the same mature plant; usually the variation is from 9 to 12 inches. In four hills raised from cuttings obtained from Oregon in 1908 the variation during 1912 and 1913[2] was from 10 to 13 inches, and an average of the 16 measurements taken (*i.e.* the minimum and maximum for each of the four hills in both seasons), which may be taken as the approximate mean length of internode, is 11·45 inches; for comparison with this 50 readings for English ♂ hops (25 minima and 25 maxima) were taken at random and the average was found to be 10·48 inches or approximately one inch less than the average for the Oregon males. This suggests that some varieties may possess a factor for long internodes, but data to hand are insufficient for a definite statement on this point. It may be interesting to note, however, in this connection that three plants (ref. nos. K 1, N 52, and O 12), each of which is a seedling of a Canterbury Whitebine *crossed with an Oregon ♂ hop,* have produced internodes 14 inches in length.

[1] Vigour in a seedling is probably often due to the stimulus resulting from fertilization.

[2] Unfortunately three of the hills succumbed to "nettle-head" disease and had to be removed before observations were resumed in 1914.

Leaves.

The length of the leaves (bracts) varies from 7 to 10 inches, rarely is 11 inches reached; the variation appears to be wholly "continuous," depending on the age and vigour of the plant and on the environment.

Colour of the leaves. The range of tint in the leaves is not extensive, though there is evidence that some plants with leaves paler than the average retain that character to a greater or less extent from year to year and when transplanted as cuttings to other parts of the garden. To determine this point absolutely may involve the use of a colour chart.

Lamina. The lamina of the leaf may be either almost flat or the margins may be more or less recurved so that the upper surface is convex, and the irregular wrinkling of the leaf, due to the relatively more vigorous development of the tissues between the veins, may be pronounced or feeble. A distinct wrinkling of the surface is usually associated with a strong recurving of the marginal portions of the leaf. The constancy of this character is, like the last, at present doubtful, yet observations tend to show that at any rate the extreme types are fairly constant.

The four Oregon male hops already mentioned have borne leaves, during the time they have been under observation, which are decidedly less recurved and wrinkled than a typical English male hop. Two English ♂ hops, D 22 [= 325 and 326] and F 2 [= 309 and 310], show the same character, all the six hills having produced leaves weakly wrinkled and recurved during the last three years without exception. Several single hills have shown the same tendency during the same period.

Some hills, relatively few in number, bear leaves which are more strongly recurved and wrinkled than the average. The plant showing this feature most conspicuously is No. 279, which is a very vigorous seedling, raised from the Fuggles crossed with an English male hop, and was planted out in the garden in 1908; only one other hill has hitherto been raised from it (viz. No. 336) and this has not been long enough established for its characters to be constant.

The lobing of the leaves. The first leaves (succeeding the cotyledons) produced by a seedling hop plant are cordate and toothed but unlobed; later three-lobed and then five-lobed leaves appear. In the English hop the five-lobed character is retained in the mature plant, the majority of the leaves of any one hill being of that form, while

three-lobed and seven-lobed leaves are comparatively of rare occurrence under the conditions of growth which obtain in the garden where these observations were made. Only one plant (No. 39) has been noticed on which the leaves are more frequently three-lobed rather than five-lobed; this feature was particularly noticeable during the past season (1914), and since the individual was planted out in 1909 it is probably a characteristic of this plant, as others of the same age and growing in the immediate neighbourhood produced five-lobed leaves.

In the Oregon hops, however, the number of lobes varies from 5 to 11, the terminal lobe of the English form being often represented by three lobes. The lobing of the leaves of these Oregon plants differs from that of the usual type in other respects. The lobes are more acuminate and the sinuses between the lobes deeper; as a result of this the lobes themselves are relatively longer. Thus the length of the terminal lobe is about three-quarters that of the whole lamina, the length of the lobe being about twice its breadth, and the width at the base (*i.e.* the distance between the two sinuses) is about half the breadth of the lobe. The corresponding dimensions for an English hop are: length of terminal lobe about two-thirds (usually less) length of lamina; breadth about *equal* to length; base *more* than half the width of the lobe.

Glands on the leaf. On the under surface of the leaves are sessile, capitate, glandular hairs, often just visible to the naked eye but easily seen with a lens. They bear a great resemblance to the lupulin glands found on the bracteoles of the ♀ inflorescence. The hop owes its bittering property to the resins secreted within the lupulin glands, and it is probable that the glands on the leaves also secrete bittering resins, since it is recorded[1] that hop leaves have been used for brewing.

To determine the variation in the number of glands on leaves of different plants it was found necessary, in order that the results should be comparative, to select portions of the leaves corresponding in position and in area. The method adopted was as follows: a circle

[1] Braungart, in *Der Hopfen* (München u. Leipzig, 1901), p. 170, writes: "Merkwürdig ist, dass man die Laubblätter des Hopfens eben wegen dieser Drüsen in Hopfennotjahren (getrocknet) schon zur Verwendung in der Brauerei, besonders zur Herstellung geringer und Nachbiere, empfohlen hat."

Prof. R. Bradley, of Cambridge University, in *The Riches of a Hop-Garden Explained* (London, 1729), wrote: "It often happens by haste, that the smaller leaves of the plant mingle with the hops. At the time of stripping these leaves are of good virtue, and were alone sold in Flanders, Anno 1566, for twenty-six shillings and eight pence a hundred, no one hop being mingled with them."

¼ inch in diameter was punched through a piece of thin sheet metal about 2 inches square; this metal plate was so placed on the leaf under examination that the circle came midway between the sinus at the base of the terminal lobe and the junction of the petiole and lamina. By means of a pocket-lens magnifying 10 diameters, the glands as a rule can easily be counted; when the glands are numerous the smaller veins are often of service, since they divide the circle into smaller areas which can be taken in order. In this way two countings could be made for any one leaf, one on each side of the mid-rib. Two leaves were selected from each hill, one at the level of the breast-wire and the other midway between that wire and the top wire; thus four readings were taken for each hill each year. In each of eleven cases 36 countings have been possible, *i.e.* where three hills have the same origin, and the general conclusions arrived at are based chiefly on the average obtained from 36 such countings in each case, although in certain typical examples fewer readings were taken, since two hills (or perhaps only one) of each were available.

Although there is often considerable variation in the number of glands borne by the leaves of a single plant, this variation (for a ¼ in. circle as indicated above) is about a certain mean which may be as high as 80 or as low as 20. Usually the number lies between 40 and 60; thus in the eleven ♂ hops from each of which 36 countings were possible the following numbers were obtained:

Reference numbers of hills	No. of glands to ¼ in. circle						Av. of 36 countings
	Lower leaves			Upper leaves			
	Min.	Max.	Av. of 18 countings	Min.	Max.	Av. of 18 countings	
A 13 [=315; 316]	51	106	75	42	94	64	69
B 11 [=298; 299]	30	87	55	33	67	51	53
C 7 [=300; 301]	41	88	59	33	65	47	53
D 22 [=325; 326]	35	70	51	32	78	47	49
F 2 [=309; 310]	18	51	34	21	36	28	31
E 10 [=311; 312]	22	76	44	25	58	37	41
E 16 [= 44; 48]	32	74	46	25	49	35	41
G 27 [=319; 320]	26	78	46	26	65	39	43
Z 12 [=292; 293]	26	64	41	25	52	44	42
30 [= 35; 42]	38	102	59	34	64	47	53
168 [=167; 169]	32	151	90	50	101	74	82

A comparison of the above figures shows two features of interest:

(1) In every case, with one exception, viz. D 22 [= 325; 326], the range of variation (difference between minimum and maximum) is greatest for the lower leaves; this was found to be the case also for the great majority of the rest of the plants examined.

(2) With the exception of Z 12 [= 292; 293] the glands are more numerous on the lower leaves than on the upper, as shown by the average of the 18 countings in each instance; this condition again obtained throughout the rest of the garden except in seven plants, where, however, the difference was not considerable as is often the case when the difference is in the other direction as in 168 [= 167; 169] of the above table.

It is advisable therefore when making observations in this connection, to select leaves at about the same level, which, for preference, should be a fairly high one.

Of all the plants examined the one with the highest average is the one appearing last in the table, viz. 168 [= 167; 169], with an average of 82; others with numerous glands are A 13 [= 315; 316] as shown in the table, and H 22 [= 26] which has given 73 as the average of 24 countings. The Oregon ♂ hops too have shown a high average, viz. 67, the maximum and minimum numbers obtained being 117 and 50 respectively.

Of those plants producing few glands the most striking are

F 2 [= 309; 310] shown in the table to have an average of 31.
269 [= 297] min. 18, max. 37, av. 28 (for 24 countings).
67 min. 16, max. 31, av. 23 (for 12 countings).

The plant giving the lowest average of all, viz. 16, is K 1, a seedling obtained by crossing a Canterbury Whitebine with an Oregon ♂.

Size of the glands. The glands are easily seen if a fresh leaf or portion of one is placed with the lower surface upwards on the stage of a microscope and examined by the aid of a $\frac{2}{3}$ in. objective. It is preferable to use reflected light only, for then the glistening yellow glands contrast well with the dark green of the leaf surface. The diameter of the glands was measured by means of an eyepiece micrometer; during 1912 the ordinary type[1] of eyepiece micrometer was employed, but during the two succeeding years the Leitz's Stufen-Mikrometer was found to be not only much more convenient, but also the divisions of the scale were more easily seen against the dark background of the leaf. In this way it was found possible to ascertain rapidly the diameter

[1] *I.e.* the type in which a glass circle with scale is dropped into the eyepiece.

of the glands correct to 5 μ. One leaf from each hill was selected and measurements taken for those glands showing extreme variation, the smallest glands which appeared to be not fully developed and those which were not more or less circular not being taken into consideration.

The variation was found to be somewhat considerable even on the same leaf; usually it is from 120 μ to 160 μ. No plant was found to have glands consistently large or consistently small, but it was seen that in general those leaves with most numerous glands showed these to be above the average in size. To illustrate this point the following table shows the minimum and maximum dimensions of the leaf-glands for those plants already mentioned as possessing numerous glands and those with few:

Reference number of plant	Av. no. of glands per $\frac{1}{4}$ in. circle	Diam. of glands	
		min.	max.
168 [=167; 169]	82	130	175
H 22 [=26]	73	130	190
A 13 [=315; 316]	69	120	170
E 12; F 7; F 9; F 17 (Oregons)	67	120	180
F 2 [=309; 310]	31	120	165
296 [=297]	28	120	165
67	23	120	150

That this is not an invariable rule is shown by the fact that occasionally where the glands are few in number they may also reach a size above the average. Two such cases have come under my observation, both plants being hybrid seedlings raised in the nursery; one is a monoecious hop (ref. no. J 2, raised from the German variety Stim $\times$ ♂ Z 12), and the other K 1 to which reference is made above. In each of these the maximum 190 μ has been reached.

The petiole. The most interesting feature connected with the petiole is the colour; this is never uniform, but (with the exception of those petioles borne by the typical pale green stems) varies on one and the same petiole from green to some shade of red. The lower side is invariably green toward the distal end but may show transitions to red towards its proximal extremity. The colour relation between the petioles and the bines on which they grow is as follows:

Colour of stem	Colour of petiole	
	Upper side	Lower side
Pale green	Green, but a little darker than lower side	Green
Green predominating but red marks fairly conspicuous	Green, tinged with red	Green
Typically mottled	Red tinged with green	Green
Red predominating but green marks fairly conspicuous	Red	Green
Dark red	Red	Green at distal end shading off to red at proximal end.

The lower side of the petiole is provided with bifid hairs similar to those of the stem; the petiole, however, is almost invariably rougher than the stem itself. Thus a bine which might be described as "almost smooth[1]" usually has a "moderately rough" petiole, while a "moderate rough" bine has a "rough" petiole.

The upper side is channelled by a single longitudinal furrow of varying depth. In the Oregons it is shallow, and the upper surface of the petiole is therefore almost flat. In the English type the furrow is more or less V-shaped in transverse section (except towards the base, where it usually tends to disappear altogether) but is often very variable even in the same plant; there is evidence, however, that in some instances there is an approach to constancy in one direction or the other. One plant represented by three hills (so making nine observations possible) has shown furrows distinctly V-shaped in section on each occasion and the same feature has been noticed in several single hills, while in others the furrow more nearly resembles that of the Oregon form.

[1] It should be noted that these terms are merely relative and no attempt has yet been made to reduce them to terms of size and frequency of the hairs.

The Laterals (Inflorescences).

By "lateral" is meant a branch growing in the axil of a leaf (bract) borne on one of the main stems (bines). The laterals above[1] the breast-wire with a few exceptions bear the staminate flowers and are thus the inflorescences. These bear bracteoles in the axils of which develop secondary laterals, which may be opposite or alternate; on long laterals they are usually opposite, on the short ones usually alternate. The utility of a male hop for pollination depends of course on the output of pollen, or, in other words, its value is according to the fertility of the laterals, and an attempt was made to determine the fertility of any plant in terms of the characters of the laterals.

Other things being equal, the longer the lateral the more flowers it bears, but factors which must be taken into consideration are the length of the internodes of the laterals and the length of the secondary laterals. Since as a rule the internodes and secondary branches decrease in length gradually towards the distal end of the lateral, it was considered sufficient to take measurements of the first internode[2] and of the longest secondary lateral.

Other characters, viz. (1) the number of nodes on the lateral at which the bracteoles were leaf-like[3], (2) whether the bracteoles were opposite or alternate, and (3) length of the stipular inflorescences[4], were also examined, as it was thought that they *might* be of varietal significance.

Of these characters, however, the only important one, so far as observations went, is length of lateral, as this is the most easily ascertained, and the other characters are dependent on that. With increase in length of lateral there is an increase in length of secondary laterals, and in length of internode; of the last two the rate of increase is not the same for both, that for the internodes being greater than that for the secondary laterals, so that a short lateral is densely flowered while a long one is lax. A typical long lateral may be contrasted with a typical short lateral as:

[1] Those laterals below the breast-wire are, together with the lower leaves, stripped off before "hop-washing" commences.

[2] By first internode is here meant that between the first and second pairs of bracteoles. When the bracteoles were not truly opposite, but were more or less alternate, the node was taken as being midway between the two bracteoles representing an opposite pair.

[3] Sooner or later along the lateral the bracteoles towards the distal end are represented merely by scales.

[4] By the "stipular inflorescences" is meant those short laterals at the base of each main lateral and growing apparently in the axils of the stipules.

Long lateral (see Fig. 1).	*Short lateral* (see Fig. 2).
Very lax	Very dense
Length 4 to 5 feet	Length 9 to 15 inches
Longest secondary lateral 6–9 in.	3–4 in.
First internode 5–7 in.	1–2 in.
Bracteoles nearly all opposite	Mostly alternate.

The development of a lateral is so easily modified by external factors that there is usually considerable variation in the same individual, and though some plants evince a tendency to produce long laterals, others short ones, no one plant has hitherto been met with possessing laterals consistently of the long or of the short type as defined above. Perhaps the best example in the garden of a ♂ hop with the tendency to produce long laterals is one growing in hills G 27, 319, 320. Those at the lower level[1] are very lax, measure 1 ft. 9 in. to 5 ft., and bear secondary laterals up to 8 in. in length; at the higher level they are less lax and from 1 ft. 6 in. to 3 ft. 6 in. in length with secondary laterals up to 9 in.

A male hop with very short dense laterals is J 36, which during the past three years has borne laterals 9 in. to 1 ft. 3 in. long at the lower level and 7 to 12 inches at the higher, with secondary laterals 3 to 4 inches long; in this particular case they are exceptionally short, and this may be due to the fact that the plant is still rather young, though it is to be remarked that the laterals were, on the whole, a little shorter in 1914 than in 1913, which seems to indicate that the plant has already reached a stage of development when length of lateral is no longer modified by the age of the plant. A typical plant bearing short laterals may be considered as one producing laterals which are in general about 15 inches long, but occasionally reaching a maximum of 2 ft. 6 in. for the lower level and 2 ft. for the higher.

The factors determining length of lateral are probably very complex but the more evident of these may here be noted. Young seedlings and hills raised from cuttings that have recently been planted have invariably short dense laterals; as the vigour of the plant increases so these inflorescences increase in length year by year until a certain maximum is attained. The laterals are usually somewhat pendant, but when they reach a suitable support they often assume the twining habit characteristic of the stem and in consequence are stimulated to increased growth. Again, should a bine lose its growing point by careless training the upper laterals grow out at an increased rate and simulate bines. Such anomalies are to be ignored when determining length of lateral.

[1] See page 178.

The pedicels and the outer surface of the perianth lobes of those plants with red bines are tinged with red, the laterals of those plants thus appearing of a reddish-green before the flowers open. An interesting feature in connection with the Oregon ♂ hops is that the laterals are more "open," *i.e.* the flowers not so crowded together as in the English form. On the re-appearance of this character in 1913 it was more closely studied and the difference was seen to be in the greater rigidity of the flowering branches of the Oregon plants, so that they stand out from the axis (primary, secondary, etc., as the case may be) bearing them more nearly at right angles than in the English forms, where the branches are more or less drooping unless supported (see Fig. 3). In the former again the pedicel is usually strongly curved at right angles immediately beneath the flower, and there is a tendency for the secondary laterals to bear the tertiary branches unilaterally. Whether the condition that obtains in the Oregons is the more advantageous for the dissemination of the pollen is not certain, but it would seem to permit of a more *uniform* distribution of the pollen than would be the case where the flowers are crowded together in a pendant tassel.

With regard to the stipular laterals there is little to be remarked. In some cases they are long and tapering (a maximum of 12 inches has been observed), in others short and rounded, while sometimes they are all suppressed with the exception of a few towards the top of the plant.

The Stipules.

These are interpetiolar; every leaf is provided with two, each of which is partially fused with the one on the same side belonging to the other leaf at that node, thus at each node there appear to be two bifid stipules alternating with the two leaves. The stipules may be upright exposing the lower surface, spreading, or recurved exposing the upper surface. In the English forms the stipules are upright to spreading, usually almost at right angles to the axis of the bine, while in the Oregon hops, although a few may be spreading the tendency is for the stipules to be so recurved that the outer surface of the tips becomes adpressed to the stem below the node.

The Flower.

Perianth. The five lobes of the perianth vary in size from $\frac{1}{8} \times \frac{1}{16}$ to $\frac{1}{10} \times \frac{1}{20}$ inch practically on every plant, but the former dimensions are the more frequently met with in the English forms, the latter in the Oregon males.

On the outer surface of the lobes are glands, which are smaller and less conspicuous than those of the leaves. The number (per each lobe) is by no means constant even on the lobes of the same flower and for any one plant is usually from 0 to 8 per lobe. Occasionally one meets with extreme types like No. 279 in which the number is frequently 15, a lobe with glands absent altogether being of very rare occurrence, and No. 83 of which the lobes are usually eglandular but may have 1 to 3 glands, no more than 3 per perianth lobe having been observed in this plant. In the case of the Oregon males the number is generally well above the average and may reach 20.

Glands of the anthers. Along the dorsal (outer) furrow of each anther may be found a number of comparatively large glands, arranged in a single row when they are few but frequently biseriate when more than 10 are present. In the English forms the number is from 0 to 8 (usually 2 to 4); 9 or 10 are rarely found and 11 were counted on one occasion only. With the exception of the last all anthers with more than 10 glands have been found only on the Oregon plants and on seedlings with an Oregon plant as one of the parents. In the Oregon males themselves the number of glands per anther varies from 4 to 18 (usually about 10) and the glands are often biseriate in the furrow. Among the English plants none has been conspicuous in producing very few glands except perhaps A 15 and I 31, in each of which the number is usually 0 to 2 and no more than 4 have been seen on an anther of either plant.

The glands of the disc. The disc from which the stamens arise is also often glandular, but this point was not noticed until too late in the season of 1912 for full observations to be made that year[1] though it was given attention during the two succeeding years. These glands are minute and individually invisible to the naked eye, therefore easily overlooked except when numerous. Here again the number often varies considerably in the same plant, from 0 to 15 being a range of variation frequently encountered at a hill; yet extreme types are to be recognized. Thus one plant (C 3) has borne glands from 10 to 25 in number, another (I 17) 5 to 25, and in these they are quite conspicuous when observed by means of a hand-lens; in the Oregon ♂ hop on the other hand no disc-glands have yet been discovered and apparently are not developed in that plant. This feature of the Oregon male is of interest since in the case of other glandiferous organs, viz. leaves, anthers,

[1] Afterwards it was found that these glands are figured in Braungart's *Der Hopfen*, p. 206.

and perianth lobes, these plants produce more numerous glands than the average plant of English origin.

The Oregon Male Hops.

A comparison of the characters of the four hills of male hops obtained from Oregon leads to the conclusion that they are all of the same variety, and that this variety differs from the English male hops in certain well-marked features. The differences drawn up in tabular form will serve to emphasize this point.

	Oregon ♂ hop	English ♂ hop
Leaves	Almost flat	Usually more or less wrinkled and margin recurved
Colour of leaves	Light green	Variable
Lobes of leaf	5–11 with tendency for terminal lobe to be trilobed	3–5 (very rarely 7), terminal lobe entire
Apex of lobes	Acuminate	Obtuse
Length of terminal lobe	About ¾ the whole length of the lamina	⅔ (usually less) length of lamina
Breadth of terminal lobe	About ½ length of lobe	About equal to length of lobe
Base of terminal lobe	Narrow; about ⅓ width of lobe	Broader, more than ½ width of lobe
Furrow of petiole	Shallow	Usually deep
Stipules	Reflexed	Upright to spreading
Glands of anthers	Usually more than 10	Usually less than 10, often absent
Disc of flower	Eglandular	Usually glandular
Perianth lobes	Usually conspicuously glandular	Glands usually 0 or few and scattered
Inflorescence	Rigid and "open"	Less rigid, drooping

The leaf-glands of the Oregon ♂ hop are generally more numerous than in the English males, though among the latter occasionally a plant is met with possessing leaves as glandular as in the former.

The differences, particularly those concerned with the stipules and the shape of the leaf, are so marked that it appears advisable to consider the Oregon form to possess specific rank distinct from *H. lupulus* L.; this point will be found discussed elsewhere[1].

Type Characters.

The observations recorded in the foregoing pages show that although in the English male hops there are certain characters which, within fairly narrow limits, are constant for the individual plant, there is

[1] Salmon, E. S. and Wormald, H.: "*Humulus Americanus* Nuttall," *Journal of Botany*, May, 1915, pp. 132–135.

between the extremes in each case a series of intervening forms—a series which is so near being perfect, that it is perhaps impossible to formulate a scheme of classification that will include and yet distinguish all the forms. At present it seems best to define the characters that may be taken as *extreme types* in each series and to employ these as standards in comparing the various forms that already exist or new forms that may arise as hybrids or as mutations. It is proposed therefore to select as type characters the two extremes in each of the four series, where (as shown in this paper) variation is considerable when applied to a large number of plants (such as were available at Wye) but confined within comparatively narrow limits when applied to the individual.

I. *Time of Flowering*[1].

(*a*) *Early*—about July 1st, and may be during last week in June or first week in July.
e.g. No. 294 [=295] June 30th–July 4th. Average July 2nd.
F 3 June 28th–July 5th. Average July 2nd.

(*b*) *Late*—about August 1st, and may be during last week in July or first week in August.
e.g. E 16 [=44; 48] July 23rd–Aug. 6th. Average July 28th.
I 31 Aug. 2nd–Aug. 9th. Average Aug. 5th.

For use in pollination it will also be necessary to select intermediate types that will be in flower when the midseason ♀ hops are in burr.

e.g. C 3 July 9th–12th. Average July 10th; early midseason.
H 22 [=26] July 16th–25th. Average July 20th; late midseason.

II. *Colour of Bine.*

(*a*) *Green, e.g.* E 16 [=44; 48].
(*b*) *Red, e.g.* H 22 [=26].

III. *Length of Laterals.*

(*a*) *Long and lax,* reaching a length of 4 or 5 feet.
e.g. E 16 [=44; 48] and G 27 [=319; 320].
(*b*) *Short and dense,* maximum length 2 feet 6 inches, but usually shorter than that.
e.g. J 36; 67; H 10.

IV. *Glands on the Leaves.*

(*a*) *Numerous*—average over 60 for ¼ in. circle taken as described on p. 183; minimum greater than 40.
e.g. H 22 [=26] with an average of 73.
168 [=167; 169] with an average of 82.

(*b*) *Few*—average under 30; maximum less than 40.
e.g. 67 with an average of 23.
296 [=297] with an average of 28.

[1] *Vide* p. 178 for definition of "Time of Flowering" as used in this paper.

Such characters as roughness of bine, colour of leaves, recurving and wrinkling of leaves, length of internodes, etc., are perhaps not sufficiently distinctive for types to be defined, but they may be useful in distinguishing varieties which show a constant feature in one or more of these characters.

The variation in the glandulation of the disc, anthers, and perianth-lobes of the flowers serves chiefly to distinguish the Oregon and the English plants, though it is possible that forms may arise among the latter which will resemble the former in one or more of these characters; some of the English plants, as already shown, approach the Oregons in their relative high number of glands on the perianth-lobes. It also provides a basis of observation when investigating the transmission of characters to the hybrid offspring obtained by crossing the two forms. Whether a male hop which produces a large number of glands will transmit that character to its progeny (male and female) is a question that only future experiments will decide, but it is presumable that such a plant would be more valuable in that direction than one in which the factors necessary for gland production are but feebly developed. Should breeding experiments prove that this is the case the value of observation concerning this character in the males will be enhanced.

In accordance with the above scheme of *type-characters* (limited to four pairs of such characters) it is conceivable that we may have 16 *type-varieties*, each of which will possess 4 of those characters, one of each pair. Of the male hops hitherto examined only one conforms to this idea of type-varieties, and but one hill of this hop (No. 67) has been available, so that the constancy of its characters is somewhat uncertain, though, since it was planted out in the garden in 1909, the features characteristic of the mature plant were probably established when observations commenced in 1912. During the three seasons 1912–13–14 it has shown the following type-characters:

I *a*, early flowering (June 26th–July 14th; av. July 5th).

II *b*, red bine.

III *b*, short laterals (8 in. to 2 ft. 6 in.).

IV *b*, few glands on leaf (16–31; av. 23).

A few plants possess three type-characters, others two, *e.g.* E 16 [= 44; 48] is late in flowering (I *b*), has a green bine (II *a*), and long laterals (III *a*); its average number of glands is 41, so that in this respect it is an intermediate form.

The extent of variation in the cultivated ♂ hop from the original ♂ of the wild *Humulus lupulus* has not been determined, as it is highly probable that many of the "wild" hop-plants now growing in this country, particularly in Kent and the other hop-growing counties, are seedlings from the varieties cultivated. Mr Salmon, however, has received from Professor P. A. Saccardo, who assures us that it has been obtained from the true wild hop in Italy, seed of *H. lupulus* labelled "Vittorio, ad sepes, *omnino sponte*, Oct. 1913. In Italia *Humulus* non colitur[1]." Plants are being raised from this seed, and when mature will be brought under observation.

Scope and Aim of Work on Male Hops.

There are two distinct lines of investigation in connection with the male hop. The first of these is concerned with the selection of those forms which are most suitable for planting commercially among the ♀ plants in hop-gardens. Obviously such plants must be selected primarily for their time of flowering, for unless this coincides with that of the ♀ plants among which they are growing they are useless, and it is evident too that vigour (including suitability for different soils) must be considered. The selection of those cuttings of male hops sent out from Wye College to hop-growers for the purpose of providing a supply of pollen is based on these two characters.

The second line of investigation is to determine how ♂ hops can be used to the best advantage in breeding operations, that is to say, how far a judicious selection of these plants for use in crossings will result in a higher percentage of commercially valuable seedling ♀ hops. This will involve (1) a comparison of the vegetative characters of the ♂ plants with those of the ♀ plants to determine which of these characters are associated with what are considered as good qualities in the ♀ plant, (2) a selection of those ♂ plants showing one or more of the characters which are considered to be useful, in order that such plants may be employed in crossing, (3) careful observation of the progeny obtained from the crosses to determine the potency of the ♂ parent in transmitting desirable qualities to the ♀ offspring.

The present paper shows that male hops exhibit definite variation in several directions and that selection for one or more characters is quite feasible.

[1] See, however, *Journ. of Bot.* May, 1915, p. 135.

Whether any pair of characters (as, for example, the type-characters enumerated above) are allelomorphic pairs is a problem the solution of which has not yet been attempted.

To the student of genetics the hop-plant is not an ideal subject, because of the comparatively long interval[1] that must inevitably elapse between the inception of two successive generations, so that it may be many years before any definite results can be obtained. Hop-growing is, however, such an important industry in this country that we cannot afford to neglect the application of modern methods in plant-breeding and selection to the hop-plant, in an endeavour to obtain new varieties superior in quality or in vigour to those already grown, particularly as this plant is now receiving the attention of scientific experts both on the continent and in America.

In conclusion I desire to express my thanks to Mr E. S. Salmon, F.L.S., of the Research Department, South-Eastern Agricultural College, whose advice during the progress of the work has been most valuable, and also to Mr F. Summers, M.Sc., Botany School, Cambridge University, who kindly made extracts from works in the library of the University.

EXPLANATION OF PLATE IV.

FIG. 1. A typical *long, lax* lateral with its subtending bract.

FIG. 2. Portion of a hop-bine bearing *short, dense* laterals.

FIG. 3. Above is shown a *rigid, open* lateral of an Oregon ♂ hop; below is the drooping type of lateral of the English forms.

[1] At Wye no seedling has been known to flower during its first season, although Dr Johs. Schmidt finds that at Copenhagen his seedlings frequently come into flower the first year (see *Comptes-rendus des travaux du Laboratoire de Carlsberg*, 11me Vol. 1915, p. 170); even during the second season the plants do not attain to their full vigour and it is evident that certain characters at any rate are not constant at that age.

(*Received May 5th*, 1915.)



Fig. 1.



Fig. 2.



THE INFLUENCE ON CROP AND SOIL OF MANURES APPLIED TO PERMANENT MEADOW.

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The observations which form the basis of the present communication have been made in connection with a comparative test of different systems of manuring meadow land which has been carried on continuously and uniformly since 1899 at the Manor Farm, Garforth[1]. Precisely similar tests were made for several years at five other centres in the West Riding of Yorkshire; the tests being continued at three centres for eight years. At Garforth the plots still continue to be manured in accordance with the original scheme.

This scheme was designed to test the following points:

(*a*) The effects of an annual dressing of dung.

(*b*) The effects of a dressing of dung every two years.

(*c*) The effects of alternate annual dressings of dung and various artificial manures.

(*d*) The effects of "complete" and "incomplete" mixtures of artificial manures.

(*e*) The comparative effects of nitrate of soda and sulphate of ammonia.

The detailed scheme of manuring is given in Table I.

The soil of these plots at Garforth is a dry, light loam, poor in lime, resting upon sandstone in the Middle Coal Measures series. The plots are each $\frac{1}{20}$th acre in extent. Dung is applied usually in March, but occasionally earlier. Superphosphate and kainit are applied usually in March, sulphate of ammonia in March to early May, and nitrate of soda in late April or early May.

[1] Experimental Farm of the University of Leeds and the Yorkshire Council for Agricultural Education.

The plots are mown when the majority are in full flower,—usually early July,—and the produce weighed as hay. The aftermath is grazed by lambs receiving cake, etc. This fact needs to be kept in mind in considering the results, as do also the relatively light annual rainfall of 20-25 inches, and the poverty of the soil in calcium carbonate.

TABLE I.

Plot No.	Manure per acre 1899 and alternate years subsequently	Manure per acre 1900 and alternate years subsequently
1	No manure	
2	Dung, 10 tons *	
3	Dung, 10 tons *	No manure
4	,,	Nitrate of soda, 1½ cwt.
5	,,	Nitrate of soda, 1½ cwt. Superphosphate, 26 %, 2 cwts.
6	,,	Nitrate of soda, 1½ cwt. Superphosphate, 2 cwts. Kainit, 3 cwts.
7	Nitrate of soda, 1½ cwt. Superphosphate, 2 cwts. Kainit, 3 cwts.	
8	Sulphate of ammonia, 130 lb. † Superphosphate, 2 cwts. Kainit, 3 cwts.	
9	Nitrate of soda, 1½ cwt. Superphosphate, 2 cwts.	
10	Sulphate of ammonia, 130 lb. † Superphosphate, 2 cwts.	
11	Nitrate of soda, 1½ cwt.	
12	Sulphate of ammonia, 130 lb. †	
13	No manure	

* For year 1913 and subsequently, only 6 tons.

† Supplying nitrogen equal in amount to that applied on Plot 7.

Effects upon Yield of Hay.

It is not proposed to discuss here the detailed records of the hay crops obtained on the different plots. The summary given in Table II will perhaps suffice to indicate the general character of the effects produced by the manuring.

TABLE II. *Yield of Hay (per acre). 16 Years (1899–1914).*

Plot No.	Maximum	Minimum	Average	Increase over average of unmanured plots	
	cwts.	cwts.	cwts.	cwts.	= per cent.
1	40¼	18	27·9±1·1 *	—	—
13	37	14½	25·7±1·1		
2	70	37½	50·6±1·6	23·8±1·9	89±7
3	57¼	28¼	44·9±1·3	18·1±1·6	68±6
4	66½	38¼	49·4±1·4	22·6±1·7	84±6
5	60¼	37½	48·8±1·1	22·0±1·4	82±5
6	67¾	36¾	50·5±1·5	23·7±1·8	88±7
7	55	29	41·2±1·3	14·4±1·6	54±6
8	51¾	20¼	35·9±1·3	9·1±1·6	34±6
9	55	28¼	39·0±1·3	12·2±1·6	45±6
10	47¼	26¾	34·2±1·1	7·4±1·4	28±5
11	49	25½	34·6±1·2	7·8±1·5	29±5½
12	47¼	20¾	29·0±1·2	2·2±1·5	8±5½

* Average yield on unmanured plots = 26·8 cwts. ± 1·0.

The main features of the average yields may be summarised as follows:

(1) With one exception (Plot 12—sulphate of ammonia alone) the manured plots all show substantial increases over the unmanured plots.

(2) The plots which have received dung (Plots 2–6)—not excepting even Plot 3, which in alternate years was entirely unmanured—show much more substantial increases than those which have received only "artificial" manures (Plots 7–12).

(3) Nitrate of soda has proved markedly superior to sulphate of ammonia (*cf.* Plots 7 and 8; 9 and 10; 11 and 12).

(4) The use of sulphate of ammonia alone (Plot 12) has proved very unsatisfactory.

(5) Within the limits of error of experiment alternate annual dressings of dung and nitrate of soda (Plot 4) have given as high yields as any other treatment (*cf.* with Plots 2, 5, 6).

(6) The indications as to the manurial needs of the soil with respect to supplies of nitrogen, phosphate and potash are as follows:

	Increased yields on	
	Nitrate plots (7, 9, 11) per cent.	Ammonia plots (8, 10, 12) per cent.
Effect of supplying nitrogen [Plots 11, 12 compared with 1, 13]	29±5½	8±5½
Effect of supplying phosphate [Plots 9, 10 compared with 11, 12]	16±8	20±7½
Effect of supplying potash [Plots 7, 8 compared with 9, 10]	9±8½	6±7½

The results indicate a marked response to nitrogenous manuring in the form of nitrate of soda, a much less sharply defined response to phosphate, and no measurable response to potash. The reasons for the poor response to nitrogen when applied in the form of ammonium salts will be dealt with later.

In considering the records of experiments of this character the averages can give only a partial indication of the real effects of the differences in treatment of the various plots. It is desirable to study also the changes step by step throughout the whole period of the experiment. It is not our intention, however, in the present communication to enter into detailed discussion of the yearly records. We would merely indicate that the yearly records show little falling-off as yet in the productivity of the unmanured plots, whilst the plots receiving dung seem to have reached practically the limits of their productive powers, giving on the average a crop which is roughly double that on the unmanured plots. This is the case even on Plot 3 (dunged and unmanured in alternate years). The records of this plot show a steady improvement in condition with the result that in the later years (prior to the reduction in the dressing of dung in 1913) the yields obtained from this plot were almost equal to those obtained from any other plot of the dung series, as the following summary shows:

Average Yields (per acre) for the Five Years 1909–13.

Plot 2	Plot 3	Plot 4	Plot 5	Plot 6
cwts.	cwts.	cwts.	cwts.	cwts.
50·8±3·0	47·3±1·8	52·0±2·6	51·5±2·2	53·2±2·8

In 1914 the yield from Plot 3 fell considerably below that from the other "dung plots," but it is, of course, too early yet to say whether this is purely a seasonal fluctuation or is to be attributed to the reduction in the dressing of dung in 1913 from 10 tons to 6 tons per acre.

Of the plots receiving "artificials" only (Plots 7–12), the nitrate plots (7, 9, 11) have, in the main, kept up the relative standard of fertility established at the start, whilst the ammonia plots (8, 10, 12) all show signs of losing ground. This is notably the case with Plot 12 (ammonia alone) which in six years of the series has given yields no greater than those obtained from the adjoining unmanured Plot 13.

Effects upon Character of Herbage.

It is now a familiar observation that the continued manuring of grass-land may produce marked changes in the character of the herbage[1]. That this has been the case in the experiments here under review is obvious to the eye and is clearly defined in the results of botanical surveys of the plots that have been made from time to time.

The first of these was made by J. N. Cameron[2] in May and June 1906, the method adopted being to take four specimen turves from each plot, tease out the different plants, sort out into species and weigh after removal of the roots. The weights were then calculated as percentages of the total herbage. The results were also checked by a rougher examination of the herbage of much larger areas of the plots. The great disadvantage of the method lay in the extreme difficulty of getting turves really representative of the whole plot, since the grasses tend to grow in patches.

A further botanical analysis of some of the plots (Nos. 1, 2, 3, 6, 7, 9, 11, 12) was made in 1909 by J. R. Bond[3], the samples this time being taken from the cut grass lying in the swaths immediately after mowing (July 22). Portions were taken from various parts of each swath, and then the whole combined sample from each plot, weighing several pounds, was dried, separated into its constituent species, and these weighed.

In order to complete the records the plots not dealt with by Bond were sampled by one of us in July 1911 and dealt with on precisely the same lines. Further analyses of some of the plots were made in 1914.

In comparing the results of the three sets of observations it must be borne in mind that the quantitative distribution of the various species that compose the flora of a meadow may vary greatly from

[1] *Cf.* Stapledon, This *Journal*, VI. 499.

[2] Results embodied in thesis submitted at Final B.Sc. Examination, 1907.

[3] Results embodied in thesis submitted at Final B.Sc. Examination, 1910. See also "Guide to Experiments at Manor Farm, Garforth," 1914, pp. 6, 7.

year to year and also at different periods of one and the same season[1]. This would account for the fact that Cameron, taking samples in May and June, found much lower proportions of the late flowering grasses, such as bent, than were found in the later analyses based upon samples taken in July. Apart from differences of this character the various observations show substantial agreement. The results of the later analyses are summarised in Table III.

TABLE III.

Name	Number of Plot											
	1	2	3	4*	5*	6	7	8*	9	10*	11	12
	%	%	%	%	%	%	%	%	%	%	%	%
1. *Lolium perenne*	·1	1·2	1·5	1·0	2·0	·7	·9	1·2	1·2	1·1	1·8	1·4
2. *Alopecurus pratensis* ...	2·8	16·1	5·3	6·1	1·6	9·0	2·0	·1	3·8	—	1·1	·4
3. *Dactylis glomerata*	3·5	14·9	12·0	5·8	10·8	19·9	29·7	4·8	26·8	5·9	27·8	12·3
4. *Avena flavescens*	8·2	4·3	34·3	20·4	24·2	20·4	31·9	7·2	13·4	1·5	7·0	·7
5. *Poa trivialis*	—	3·6	1·9	·1	—	2·0	·1	·6	·2	·5	—	—
6. *Poa pratensis*	·2	—	·1	·1	—	·1	·2	·4	·1	·4	·2	·5
7. *Festuca ovina et duriuscula*	1·9	·1	1·3	1·9	·5	·9	4·6	3·0	2·6	5·1	7·4	1·1
8. *Arrhenatherum avenaceum*	1·1	3·1	3·8	1·4	1·6	9·8	·2	·2	8·4	—	·1	·5
9. *Anthoxanthum odoratum.*	3·3	—	·2	·9	·2	·02	·2	14·2	·8	8·5	1·1	4·5
10. *Agrostis vulgaris*	53·4	·2	1·4	3·7	1·9	1·1	12·6	58·8	9·0	71·4	23·4	57·3
11. *Bromus mollis*	—	13·6	1·9	14·3	32·8	2·7	·3	·2	·7	·1	·4	·1
12. *Holcus lanatus*	2·4	·6	3·2	6·2	·7	·8	2·4	5·0	2·2	2·1	1·7	1·1
13 *Rumex acetosa*	15·4	16·1	24·3	34·4	21·1	18·6	9·1	1·4	20·7	2·3	14·7	10·2
14. *Heracleum sphondylium* .	·5	10·3	·5	1·1	1·6	10·2	·8	·3	2·1	—	2·0	·5
15. *Anthriscus sylvestris*	—	10·2	3·5	—	1·2	·3	—	·2	—	—	—	—
16. *Bunium flexuosum*	2·2	·1	·4	—	·1	·4	·9	·7	1·8	·2	2·1	1·1
17. *Luzula campestris*	·2	—	·01	—	—	—	·03	·4	·01	·2	·3	·5
18. Various other weeds ...	·03	·05	·2	·1	·04	·1	—	1·2	·01	·1	·1	·1
19. *Lathyrus pratensis*	—	—	·1	1·0	—	—	·3	—	—	—	—	—
20. *Trifolium pratense*.....	—	—	—	1·0	—	—	—	—	—	·03	·01	—
21. Undetermined	4·8	5·6	4·1	·5	·7	3·0	3·8	·1	6·2	·6	8·8	7·7

* Analysed in 1911 by Ruston. All other plots analysed in 1909 by Bond.

It will be noted that the dominant grass on the unmanured land (Plots 1 and 13) is *Agrostis vulgaris*, but that this has been very largely suppressed on the dunged plots (2–6). It persists to a considerable extent on the plots manured with artificials only (7–12), being much less pronounced on the nitrate plots (7, 9, 11) than on the ammonia plots (8, 10, 12). *Rumex acetosa* is prominent throughout and is increased rather than diminished on the dunged plots. It is least

[1] *Cf.* Armstrong, This *Journal*, 1907. II. 299.

pronounced on the plots receiving "complete" artificials. Other well-defined changes effected by the application of dung are the encouragement of *Dactylis glomerata, Alopecurus pratensis, Avena flavescens, Bromus mollis* and the large umbelliferous weeds *Heracleum sphondylium* and *Anthriscus sylvestris*.

On the "artificials" plots (7–12) *Dactylis glomerata* and *Agrostis vulgaris* are conspicuous grasses, whilst *Rumex acetosa* is a prominent weed; *Alopecurus pratensis* and *Bromus mollis* have made no headway, whilst *Avena flavescens* is prominent only on Plot 7 (nitrate, superphosphate, kainit) and Plot 9 (nitrate, superphosphate); the umbelliferous weeds are practically absent.

On comparing the nitrate plots (7, 9, 11) with the ammonia plots (8, 10, 12) it will be seen that the use of nitrate has apparently encouraged, or at least sustained, *Alopecurus pratensis, Dactylis glomerata, Avena flavescens*, and *Rumex acetosa* and has repressed *Agrostis vulgaris*. It is interesting to note that the use of ammonium sulphate along with superphosphate (Plots 8 and 10) has very greatly checked the growth of *Rumex*—a weed whose presence in appreciable amount is commonly regarded as indicative of acidity or "sourness" in the soil.

The general character of the herbage may perhaps be conveniently summarised as follows, using a purely arbitrary classification of the grasses:

Plot No.	Good grasses (Nos. 1–8, Table III)	Inferior grasses (Nos. 9–12, Table III)	Leguminous plants (Nos. 19 and 20, Table III)	Miscellaneous weeds (Nos. 13–18, Table III)
	per cent.	per cent.	per cent.	per cent.
1	17·8	59·1	—	18·3
2	43·3 (36·4 †)	14·3 (21·4)	— (3·0)	36·8 (34·1)
3	60·2	6·8	·1	28·9
4 *	37·7	24·2	1·1	35·6
5 *	40·9	35·0	—	24·0
6	62·7	4·6	—	29·5
7	69·6 (75·3)	15·5 (14·1)	·3 (2·0)	10·8 (8·6)
8 *	31·7 (11·7)	63·9 (78·4)	— (0·4)	4·1 (8·0)
9	56·6	12·7	—	24·5
10 *	22·9	73·6	·03	2·8
11	45·4	26·7	·01	19·2
12	17·0 (1·8)	62·9 (87·4)	— (—)	12·5 (10·6)
13 *	12·9 (3·4 ‡)	75·7 (90·2 ‡)	·05 (0·1 ‡)	11·1 (4·7 ‡)

* See note to Table III.

† Figures in brackets refer to samples taken in 1914.

‡ Averages of Plots 1 and 13 in 1914 crop.

The preponderance of "inferior grasses" on the unmanured plots (1 and 13) and on the plots dressed with sulphate of ammonia (8, 10, 12) is brought out very clearly in this summary.

As indicated in the table a comparison is possible in the case of five plots of the composition of the herbage in two different years, viz., 1909 (or 1911 for Plots 8 and 13) and 1914.

The unmanured plots (1 and 13) show a marked increase in the inferior grasses, which the detailed results (not given here) show to be due to a great development of bent (*Agrostis vulgaris*).

The annually dunged plot (Plot 2) shows no great alteration in the proportions of the different groups, but there have been changes within the groups. Thus in 1914 the proportion of cocksfoot (*Dactylis glomerata*) was much lower, and of golden oat grass (*Avena flavescens*) much higher than in 1909. Of the inferior grasses Yorkshire fog (*Holcus lanatus*) had increased considerably.

On the plot receiving annual dressings of nitrate, superphosphate, and kainit (Plot 7) the general distribution was much the same in the two crops but cocksfoot was lowered and rye-grass increased. Bent was appreciably more abundant as were the umbelliferous weeds.

On the two plots receiving sulphate of ammonia (Plots 8 and 12) the composition of the two years' crops was greatly different, the later crop containing a much smaller proportion of the better grasses, notably cocksfoot, golden oat grass and the fescues, whilst the inferior grasses, notably bent and Yorkshire fog, were correspondingly increased. In agreement with the indications of Table III, sorrel (*Rumex acetosa*) was considerably reduced on these plots.

A further point of interest brought out in the earlier work of Cameron is the variation in the proportion of dead grass leaves that persist about the bases of the plants. Estimations made by him of the proportion of dead grasses to total herbage on each plot resulted as follows:

Plot	%	Plot	%
1	21·6	8	29·7
2	4·6	9	8·9
3	9·9	10	28·0
4	8·5	11	9·2
5	11·7	12	33·5
6	11·6	13	22·8
7	13·1		

Attention may be specially directed to the high proportions on the unmanured plots and especially on the "ammonia plots" (8, 10, 12). The point is one of considerable interest to which we shall return later.

Chemical Composition and Estimated Nutritive Value of the Hay Crop.

In order to arrive at a complete record of the effects of the different manurings upon the hay crop it is necessary to know not only the yields of hay but also the relative nutritive value of the produce from each plot. This latter can be assessed either directly by feeding experiments or indirectly from the chemical composition of the hays. In the case

TABLE IV. *Chemical Composition of Hay.*

Plot No.	Crude protein	True protein	Digestible true protein *	Crude fibre	Ash	Ether extract	N-free extractives	Moisture in hay †
	Per cent. in dry matter							%
				1909 crop				
1	8·1	—	—	28·9	5·3	57·7		
2	9·3	—	—	32·0	6·5	52·2		
3	8·7	—	—	31·3	5·8	54·2		
7	9·5	—	—	26·4	6·2	57·9		
9	9·8	—	—	29·2	6·2	54·8		
11	10·4	—	—	28·7	6·4	54·5		
12	11·6	—	—	27·8	5·8	54·8		
				1911 Crop				
1	8·1	6·8	5·1	29·0	6·3	3·1	53·5	7·9
2	9·0	7·0	5·3	36·2	8·6	3·4	42·8	7·1
3	8·7	7·2	5·4	33·3	7·3	3·2	47·5	9·3
4	9·0	7·5	5·7	32·6	8·2	2·7	47·5	9·4
5	8·8	8·0	4·9	30·6	7·2	3·2	50·2	8·9
6	8·6	7·2	5·0	30·5	7·6	3·7	49·6	9·4
7	8·3	7·8	6·1	24·8	7·1	3·0	56·8	6·6
8	8·7	6·7	4·6	29·7	6·7	2·0	52·9	9·1
9	9·5	7·8	6·2	28·2	7·5	4·3	50·5	10·9
10	9·6	7·4	5·7	28·3	6·2	2·6	53·3	9·0
11	10·7	8·2	6·1	26·3	6·8	3·1	53·1	9·8
12	11·1	7·7	5·6	28·2	5·8	2·2	52·7	10·7
13	8·4	6·9	4·8	28·5	6·5	2·5	54·1	10·1
				1914 Crop				
1 and 13	9·3	7·7	4·4	26·1	6·5	3·5	54·6	8·6
2	10·4	7·8	3·6	31·0	7·2	3·4	48·0	9·4
7	9·5	8·6	4·5	24·9	6·7	3·9	55·0	8·4
8	9·6	8·3	4·0	28·1	6·5	3·2	52·6	8·5
12	11·8	9·3	4·6	27·0	6·4	3·0	51·8	9·7

* Laboratory determinations with acid solution of pepsin. † At time of analysis.

of the Garforth experiment the size of plot used precluded the application of the direct method, so that recourse could only be had to chemical analysis. Various observers[1] have shown how unreliable are the indications of present methods of analysis as a guide to the relative feeding

[1] *e.g.* Hall and Russell, This *Journal*, 1912, IV. 339.

values of different pastures, but it is not unreasonable to expect that in the case, such as the present, of adjoining plots on a small area of one and the same field, the indications will be more reliable.

The produce of some of the plots was sampled and analysed by Bond in 1909. Two years later (1911) the crops on all the plots were again sampled and analysed by us. On five plots further sampling and analysis took place in 1914. In every instance the samples were taken at the time of cutting, and dried entirely under cover. The results of the analyses are summarised in Table IV.

The results of the three series of analyses show fair agreement in general, the following points being common to each:

(1) With but one exception (1911 Crop, Plot 7) the proportion of crude protein is greater on the manured than on the unmanured plots (1 and 13).

(2) The enrichment in crude protein is greatest on those plots where nitrogenous manure alone was applied (Plots 11 and 12).

(3) The proportion of crude protein is lower on the plots manured with nitrate of soda (7, 9, 11) than on the corresponding ammonia plots (8, 10, 12).

(4) The differences above referred to are due rather to the non-protein than to the true protein fraction of the crude protein.

(5) The proportion of crude fibre is markedly higher on the dunged plots than on the rest.

(6) The lowest proportion of crude fibre and the highest proportion of nitrogen-free extractives ("soluble carbohydrates") are found on the plot receiving a "complete" mixture of artificials including nitrogen in the form of nitrate of soda (Plot 7).

(7) The proportion of ash in the hay grown with nitrate of soda (7, 9, 11) is higher than that in the hay from the corresponding ammonia plots (8, 10, 12).

In interpreting the results both here and elsewhere it must be borne in mind that the hay on the dung plots was dead ripe at the time of cutting. This reveals itself more particularly in the increase of crude fibre. Late cutting and the presence of a relatively large proportion of tall, coarse grasses and weeds all tend to give a hay containing a high proportion of crude fibre. These are the conditions which obtained on the dunged plots.

In assessing the nutritive value of the produce from each plot it is necessary to take into account the digestibility of the material.

This we can only arrive at in the present instance by indirect means since direct digestion trials with animals could not be carried out. The procedure adopted for our present purpose is that followed by us in a previous paper[1].

The digestible protein, as arrived at in the laboratory, is given in Table IV. The "amides" are assumed to be completely digestible. The sum of digestible "carbohydrates" and fibre is taken as equal to the total "carbohydrates" (Henneberg and Stohmann's Rule). The "ether extract" is assumed to be one-half digestible. The results are summarised in Table V.

TABLE V. *Estimated Digestible Constituents of Hay.*

Plot No.	Per cent. in dry matter				Total yield per acre			
	True protein	"Amides" (non-protein N × 6¼)	Ether extract	"Carbohydrate" and fibre	True protein	"Amides"	Ether extract	"Carbohydrate" and fibre
	%	%	%	%	lb.	lb.	lb.	lb.
				1911 Crop				
1	5·6	1·3	1·5	53·5	145	35	40	1393
2	5·7	2·0	1·7	42·8	248	84	74	1846
3	5·9	1·6	1·6	47·5	285	78	78	2287
4	6·2	1·5	1·3	47·5	293	72	63	2218
5	5·3	0·8	1·6	50·2	255	39	77	2393
6	5·5	1·4	1·8	49·6	264	67	90	2400
7	6·6	0·5	1·5	56·8	233	18	53	2021
8	5·0	2·0	1·0	52·9	139	55	28	1500
9	7·0	1·7	2·1	50·5	217	49	64	1504
10	6·2	2·2	1·3	53·3	171	29	35	1449
11	6·7	2·5	1·5	53·1	180	67	29	1395
12	6·2	3·4	1·1	52·7	130	70	23	1093
13	5·4	1·4	1·2	54·1	111	28	26	1101
				1914 Crop				
1 and 13	4·4	1·5	1·6	49·9	81	28	29	915
2	3·6	2·3	1·5	43·5	151	97	63	1827
7	4·5	0·6	1·8	50·4	146	18	58	1637
8	4·0	2·2	1·4	48·2	91	50	32	1093
12	4·6	2·3	1·3	46·7	111	55	31	1124

From the data in Table V we have calculated the "starch equivalents" of each crop, proceeding upon the lines laid down by

[1] This *Journal*, 1912, IV. 305.

Kellner[1], as outlined in our earlier paper[2]. The results are embodied in Table VI.

TABLE VI. *Estimated Starch Equivalents of Crops from Different Plots.*

Plot No.	Per 100 lb. dry matter		Total per acre		Relative values of crops (unmanured = 100)		
						In terms of starch equivalents	
	For maintenance	For production	For maintenance	For production	From weights of hay alone	For maintenance	For production
	lb. (1)	lb. (2)	lb. (3)	lb. (4)	(5)	(6)	(7)
				1911 Crop			
1 and 13	58	39	1334	897	100	100	100
2	51	27	2117	1121	180	159	125
3	54	33	2552	1559	206	191	174
4	53	33	2439	1518	200	183	169
5	55	37	2572	1730	203	193	193
6	55	36	2613	1710	207	196	191
7	64	47	2173	1598	148	163	178
8	56	39	1526	1063	118	114	119
9	57	39	1696	1160	131	127	129
10	58	40	1595	1100	120	120	123
11	59	42	1564	1113	115	117	124
12	57	39	1183	810	90	89	90
				1914 Crop			
1 and 13	59	42	1082	770	100	100	100
2	52	33	2183	1386	227	202	180
7	60	44	1949	1430	176	180	186
8	57	39	1293	885	123	119	115
12	56	38	1348	915	130	125	119

It will be seen that when the estimated nutritive value of the hay is taken into account (cols. 6, 7) the dung plots (2–6) do not show quite so great an advantage over the others as when judged by weight of hay alone (col. 5).

In the two years to which the data refer the crop of highest feeding value per 100 lb. (Table VI, cols. 1, 2) was evidently that from Plot 7 ("complete" artificials including nitrate of soda). When both yield and quality are taken into account (cols. 3, 4) the best results (1911 crop) are shown by Plots 5 and 6 which received dung and artificials

[1] *Scientific Feeding of Farm Animals*, pp. 82–93. [2] *loc. cit.* p. 311.

alternately (dung for 1911 crop). Of the five plots sampled in 1914, the "continuous dung" plot (Plot 2) compares unfavourably with Plot 7 in feeding value per acre. On examining further the records of this plot (dung annually) it is noted that in each year although the crop on this plot was much heavier than that on Plot 7, it was generally so inferior in feeding value that the actual value per acre was distinctly less.

Removal of Manurial Ingredients by Crop.

In addition to the ordinary analysis of the hays from the plots in 1911, determinations were made of the phosphoric acid, potash and lime present in them, with the results summarised in Table VII.

TABLE VII. *Manurial Ingredients present in Hay* (1911).

Plot No.	Nitrogen	Phosphoric acid (P_2O_5)	Potash (K_2O)	Lime (CaO)	Ratio $CaO : P_2O_5$
	%	%	%	%	
1	1·20	0·36	0·62	0·43 *	1·16 : 1 *
2	1·34	·57	2·25	·53	0·93 : 1
3	1·27	·51	2·18	·59	1·16 : 1
4	1·31	·67	2·26	·67	1 : 1
5	1·28	·56	1·87	·66	1·18 : 1
6	1·25	·51	1·54	·64	1·26 : 1
7	1·25	·54	1·28	·65	1·20 : 1
8	1·26	·50	1·50	·50	1 : 1
9	1·35	·52	0·63	·73	1·41 : 1
10	1·40	·94	0·76	·66	0·70 : 1
11	1·54	·41	1·12	·46	1·12 : 1
12	1·58	·36	·75	·44	1·22 : 1
13	1·21	·39	·68	—	—

* Average of Plots 1 and 13.

The data show clearly that, with perhaps the exception of the potash, the composition of the hay with regard to manurial ingredients is a very uncertain guide to the actual manurial treatment. The phosphoric acid figures show far less variation than the potash, whilst the nitrogen varied least of all. The proportions of lime are uniformly low in accordance with the poverty of the soil in this ingredient. More especially are the ratios of lime to phosphoric acid much below those taken as normal for meadow hay ($2–2\frac{1}{2} : 1$). The point is of interest as having a possible bearing upon the feeding value of the grass[1].

[1] *Cf.* Ingle, This *Journal*, 1910, III. 22.

Influence upon Chemical Composition of Soil.

Analyses were also made in 1911 of the soils of the different plots and it is of interest to compare the results obtained with the soils with those given above for the crops. The essential data are summarised in Table VIII[1].

TABLE VIII. *"Available" Plant Food in Soils.*

Plot No.	"Available" phosphoric acid	"Available" potash	Calcium carbonate
	%	%	%
1 and 13	0·012	0·007	0·06
2	·028	·051	·14
3	·019	·025	·16
4	·023	·043	·21
5	·016	·036	·11
6	·015	·043	·15
7	·018	·027	·14
8	·019	·025	·08
9	·013	·010	·16
10	·015	·022	·06
11	·013	·008	·11
12	·010	·015	·01

It will be found on comparison that there is a fair degree of correlation between Tables VII and VIII, as, for example, in the accumulation of potash on the dunged plots and its scarcity on Plots 1 and 9–13. It is further of interest to note the depletion of the supplies of available potash on Plots 9 and 11 which receive nitrate of soda as compared with the corresponding ammonia plots (10, 12).

There is little sign of correlation with regard to phosphoric acid. The lowest proportion is recorded on Plot 12 (ammonium salts alone), a fact which is in accord with the known power of ammonium salts of facilitating the taking up of phosphates by plants.

The general lowness of the supplies of calcium carbonate has been referred to already. It will be observed how the use of sulphate of ammonia on Plots 8, 10 and 12 has accentuated this poverty.

The indications of soil analysis with regard to the calcium carbonate were borne out by the results of determinations of the "lime-fixing" powers of the soils of the plots, made by shaking up separate equal

[1] The chemical composition of the soils was more exhaustively studied but it is not proposed to deal with the results here.

TABLE IX. *Showing Volume of Acid (N/10) required to neutralise "unfixed" Lime remaining after Treatment of Soil with increasing Proportions of Lime.*

Lime	Plot 2	Plot 7	Plot 8	Plot 12
%	c.c.	c.c.	c.c.	c.c.
·25	1·4	2·5	1·2	0·5
·5	2·8	5·0	2·6	0·8
·75	5·2	7·6	4·1	2·0
1·0	8·4	10·3	5·0	2·5
1·25	9·8	15·2	6·8	4·0
1·5	11·0	15·4	8·6	5·2
1·75	17·6	21·6	10	7·3
2·0	35·3	40·1	20·2	10

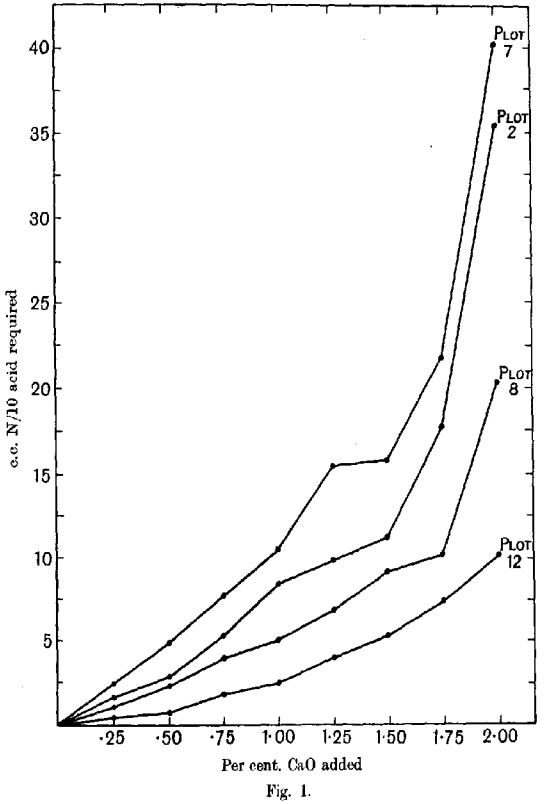


Fig. 1.

portions (100 gms.) of soil with water and varying amounts of lime, and determining the lime left free after twenty-four hours.

It was found that the soil of Plot 12 "fixed" practically the whole of the lime applied even when the proportion was as high as two per cent. of the soil used, whilst even in the most favourable case the soil fixed practically 1·5 per cent. of its weight of lime before any appreciable surplus of free lime remained. The data for a few plots are given in Table IX (*cf.* also Fig. 1).

Influence upon Bacterial Activities in Soil.

The part played by soil bacteria in determining fertility, more particularly by regulating the supply of available plant food, is now common knowledge and need not be enlarged upon. In this connection interest centres mainly round the supply of nitrogen, which is presented by the bacteria first in the form of ammonia and then, if the conditions are favourable, the latter is further converted to nitrates. Under normal conditions this latter change ("nitrification") goes on more rapidly than ammonia production and hence the amount of ammonia present in the soil at a given time is usually exceedingly small. In the case of ordinary arable soils investigated by Russell[1] the ammonia amounted to only 1–2 parts per million of soil, rising to 3–4 parts in the case of rich dunged soils and garden soils. Much higher proportions were found, however, in the soils of the Garforth hay plots here under review, as may be seen in Table X.

TABLE X. *Nitrogen as Ammonia and Nitrate in Soils* (1911).

Plot No.	Total nitrogen	Nitrogen present in form of ammonia	Nitrogen present in form of nitrate
	Parts per million	Parts per million	Parts per million
1 and 13	1640	4·0	3·2
2	2490	15·1	7·6
3	2300	7·3	11·3
4	2200	7·6	10·5
5	1910	9·8	8·7
6	1940	11·6	7·4
7	1840	15·6	6·1
8	1870	16·4	1·6
9	1760	12·8	4·3
10	1940	18·1	1·2
11	2100	12·6	5·6
12	1860	7·1	0·2

[1] Russell, This *Journal*, 1910, III. 233.

The proportions of nitrogen (total) in the soils are much what one would expect from the manuring, the dung plots (2–6) having accumulated considerable reserves.

The ammonia results can be explained by assuming that the conditions for nitrification were much more unfavourable than in the soils examined by Russell. This is highly probable in view of the low proportions of calcium carbonate in the soils. The case of Plot 12 (ammonium salts alone) is specially interesting, the relatively low proportion of ammonia in this soil as compared with Plots 8 and 10 indicating that on this plot the conditions are becoming unfavourable even for production of ammonia (see later).

The nitrates present in the soils at the time of analysis were in no case high, and indeed on the ammonia plots were hardly measurable.

Further evidence of the low bacterial activity on some of the plots is furnished by the presence (*cf.* p. 204) round the bases of the grasses of a decided mat of undecayed vegetable matter, this being notably the case on Plots 8, 10, and 12. That this acts as a handicap to the grasses by withholding water from their roots was illustrated by samples of some of the soils taken to a depth of 9 inches on September 17, 1912. The determinations of moisture gave the following results:

Plot	Moisture
12	16·7 per cent.
10	17·8 „
8	19·2 „
7	21·7 „
3	24·7 „
2	28·6 „

The difference of 12 per cent. between Plots 2 and 12 will represent roughly a difference of 120 tons of water in the supply per acre, equivalent to a rainfall of $1\frac{1}{4}$ inch. Similar observations in a neighbouring pasture with limed and unlimed plots—the unlimed plot having a thick "mat"—lead to the same conclusions and indicate further the beneficial effect of the liming upon bacterial activity. These results are summarised below:

Plot	Date of test	Moisture in soil
Unlimed	Nov. 9, 1911	8·1 per cent.
Limed	„ „	21·5 „
Unlimed	Oct. 30, 1913	11·1 „
Limed	„ „	18·7 „

The indirect evidence of marked differences in bacterial activity on the different plots was fully borne out by direct determinations, made in 1911, the results of which are summarised in Table XI.

TABLE XI. *Bacterial Activity in Soils.*

Plot No.	Total no. of bacteria* per ·0001 gm. soil	Relative activity on different plots (unmanured = 100)			Relative activity of Catalase ferment‖ (unmanured = 100)	Undecayed grasses (per cent. of total herbage) (Cameron) *cf.* p. 213	Moisture content of soils on Sept. 17, 1912
		Ammonia-producing bacteria†	Nitrate-producing bacteria‡	Nitrogen-fixing bacteria§			
1 and 13	42	100	100	100	100	22·2	21·5 %
2	192	107	383	236	233	4·6	28·6
3	108	104	323	168	186	9·9	24·7
4	112	103	246	119	177	8·5	25·0
5	117	101	283	100	163	11·7	25·3
6	128	103	217	100	167	11·6	24·2
7	82	99	263	81	87	13·1	21·7
8	62	89	79	75	77	29·7	19·2
9	45	99	142	75	99	8·9	22·8
10	16	88	31	49	91	28·0	17·9
11	32	95	94	63	80	9·2	20·5
12	4	82	13	37	61	33·5	16·7

* Colonies on Agar incubated 3 days at 38° C.
† After 3 days' incubation (5 gms. soil).
‡ „ 21 „ „ at 28° C. (5 gms. soil).
§ „ 7 „ „ (5 gms. soil).
‖ „ 3 hours' „ at 28° C. (5 gms. soil).

It will be observed that, with the exception of the production of nitrates on Plot 7, the number and activity of the organisms were relatively greater on the dunged plots than on the plots which were either unmanured or received only "artificials." Further that bacterial activity of all kinds was much greater on the nitrate plots (7, 9, 11) than on the corresponding ammonia plots (8, 10, 12). The adverse conditions for bacterial life on Plot 12 are strikingly exemplified. The soils which showed the lowest bacterial activity (8, 10, 12) showed also the lowest moisture-content and carried the biggest proportion of dead undecayed grasses (*cf.* p. 213).

The differences would appear to be least pronounced with regard to the ammonia-producing organisms, but further tests showed that the data recorded in the table scarcely give a fair indication of the real differences in ammonia-producing power of the different plots. The

data given in the table were arrived at by incubating 5 gms. of soil for three days at 28° C. with 80 c.c. of a nutrient solution containing 10 grms. of peptone per litre. In view of the small differences in ammonia production thus obtained, further estimations were made with varying amounts of peptone and different periods of incubation. The general character of the results is illustrated by Fig. 2. It will be seen that

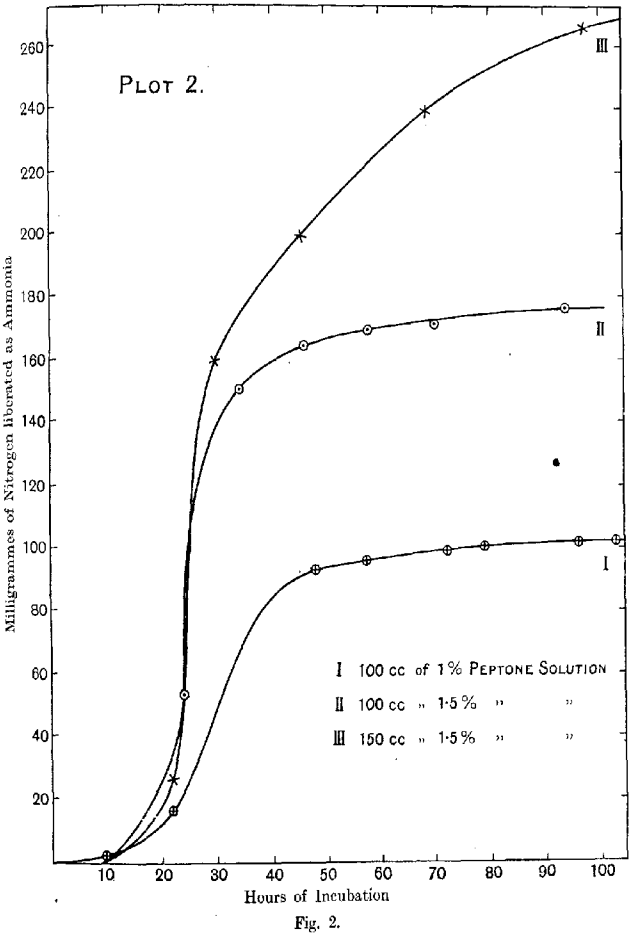


Fig. 2.

the amount of ammonia produced is greatly influenced by the supply of peptone, but that in all cases the rate of production falls greatly after about 50–60 hours' incubation. This latter conclusion was in the main borne out by similar tests with other soils, but deviations from the general rule were not lacking.

On repeating now the comparative tests with the different soils, using 100 c.c. of the 1 per cent. peptone solution and estimating the ammonia present after different periods of incubation, much more marked differences were found than those recorded in Table XI. The following comparison of the best and worst plots will serve as an illustration (see also Fig. 3). For convenience of comparison the amount

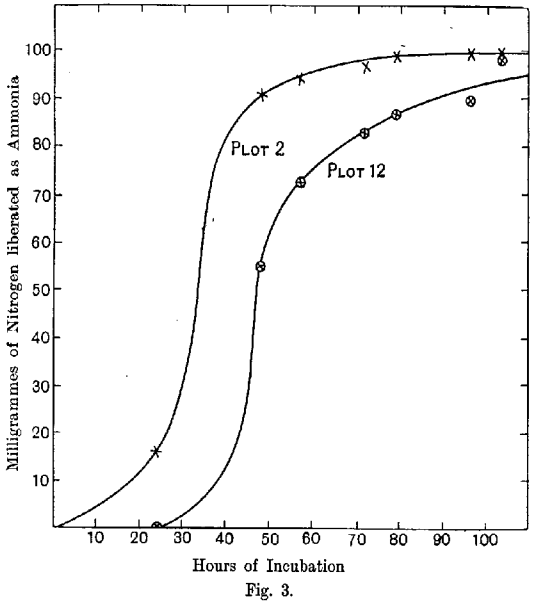


Fig. 3.

of ammonia produced by Plot 12 after 48 hours' incubation is taken as 100. The figures in brackets give the amounts for Plot 2, taking the amount for Plot 12 for the same period as 100.

Relative Amounts of Ammonia produced during

	24 hrs.	48 hrs.	57 hrs.	72 hrs.	79 hrs.	96 hrs.	103 hrs.
Plot 2	29	164	171 (130)	175 (117)	179 (114)	180 (111)	180 (101)
Plot 12	Nil	100	132	150	157	162	179

Thus at the end of 48 hours the ammonia output on Plot 2 was 64 per cent. more than on Plot 12, whilst by the end of the third day (72 hours) the superiority had been reduced to 17 per cent., and after 103 hours the two plots showed practically equal records. Had we been able to establish conditions such as prevail on the plots, where the ammonia is largely removed as it is produced, the superiority shown by Plot 2 in the earlier stages would doubtless have been maintained throughout. It will be noted further that whereas the maximum output in the case of the soil from Plot 2 was practically reached in 48 hours, more than twice this period was required in the case of Plot 12.

Generally speaking, the ammonia-production was decidedly more active on the dunged plots (Plots 2–6) than on the "artificials" plots (Plots 7–12). This is exemplified by the following data obtained by incubating 5-gram portions of the different soils with 100 c.c. of 1·5 per cent. peptone solution.

Milligrams of Ammonia produced during

	9 hrs.	24 hrs.	34 hrs.	46 hrs.	58 hrs.	70 hrs.	94 hrs.
Plot 2	1·5	53·9	150·5	164·6	168·8	170·5	175·7
„ 7	·8	14·2	100·3	129·2	130·0	147·4	165·4
„ 8	Nil	6·8	39·5	120·3	112·8	130·3	163·4

The superiority of the dunged plot (Plot 2) throughout the first half of the incubation period is seen to be very marked, whilst Plot 7 (complete artificials including nitrate of soda) shows to advantage in the earlier stages in comparison with Plot 8 (complete artificials, including sulphate of ammonia). This latter difference was reproduced also in the comparison of the other nitrate plots (9, 11) with the corresponding ammonia plots (10, 12). The poverty in respect of calcium carbonate of the soil throughout the plots is doubtless largely responsible for this relatively unfavourable biological condition of the ammonia plots.

SUMMARY.

The experiments reviewed in the preceding pages have been carried out on a light loam soil very poor in lime, in a district of medium rainfall (20–25 inches).

The chief conclusions drawn from the results are as follows:

1. Although the heaviest crops have been obtained with an annual application of dung, they are little heavier, and more costly to obtain, than the crops obtained with a biennial application of dung, especially

if in the alternate year a light dressing of "artificials" including nitrate of soda be given.

2. A complete mixture of "artificials," including nitrate of soda, has given good average crops, but not equal to those given by a biennial application of dung.

3. For the soil and other conditions of Garforth nitrate of soda is distinctly better for the hay crop than sulphate of ammonia. This is doubtless largely associated with the poverty of the soil in calcium carbonate.

4. The different manurings have effected marked and characteristic changes in the botanical composition of the herbage. In particular the continued use of ammonium salts has led to serious deterioration.

5. There are now differences also in the chemical composition of the herbage, which probably represent substantial differences in feeding value. For equal weights, the hay grown with dung appears to have a lower feeding value than that grown with a good mixture of "artificials."

6. The composition of the ash of the hay does not reflect the character of the manuring, except with regard to potash.

7. Substantial changes in the power of the soil to supply plant food have taken place as a result of the manuring. The most marked effect is the removal of carbonate of lime by the prolonged use of ammonium salts.

8. The effects—direct and indirect—of the manuring upon the soil have led to marked differences in bacterial activity. In some cases the reduction in biological activity is so great that dead grasses accumulate and form a mat on the soil so thick that the penetration of water to the lower layers is seriously impeded.

9. In comparing the ammonia-producing powers of soils it is desirable that estimations of ammonia produced be made at relatively short intervals, say every twelve hours, since the *rate* of production in the earlier stages is a more characteristic index of the activity of the soil than the *total output* after prolonged incubation.

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THE THEORY OF WETTING, AND THE DETERMINATION OF THE WETTING POWER OF DIPPING AND SPRAYING FLUIDS CONTAINING A SOAP BASIS.

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The dipping of sheep and cattle, as a means of eradicating 'scab,' lice, ticks, etc., and the diseases which it is now known the latter may transmit, has met with such success, that compulsory dipping is now in vogue in most pastoral countries. Where compulsory dipping obtains, there must of necessity be some system of the standardisation of dips. In Queensland and South Africa, the respective Governments issue official formulae from which the stockbreeder can prepare his own dipping fluid. Only such proprietary dips, as are duly recognised by the Government, may be employed. In the United States, the regulations for the sale of proprietary dips are still more stringent. The quantity of active substance, usually sodium arsenite, nicotine or cresylic acid, is defined within very narrow limits. Further, no proprietary dip is now recognised, unless the manufacturer can furnish a 'Field Tester,' by means of which the stockbreeder can himself determine, in a simple and fairly trustworthy manner, the percentage of active constituent in his bath.

The underlying idea, upon which all these regulations are based, is that the principal factor, which determines the efficacy of a dipping fluid, is the quantity of *toxic* substance present. It has been shown, however, in experience derived from field experiments and routine dippings, that this assumption is unwarranted. There is another factor of almost equal importance, *viz.*, the capacity of the dipping fluid to *wet* the greasy hide or fleece. A plain aqueous solution of sodium arsenite, containing 1 part of arsenious oxide in 600 parts of

water, may be quite ineffective as a dip, whilst a solution of the same concentration, but containing in addition a small percentage of soap and oil to increase the wetting power, proves to be quite satisfactory. The fact that the Queensland official dip contains soap as a basis may be quoted as an indication that the importance of the wetting power of dipping fluids is recognised in actual practice.

The difficulty of determining the wetting power of a solution has, however, precluded this point being considered in the comparison of dips in the laboratory.

What has been said of dipping fluids, refers with equal force to horticultural spraying fluids—a high percentage of *toxic* substance is no criterion of the efficiency. The supreme importance of the spray fluid having a *high wetting power* is gradually becoming more and more recognised; though not generally by Government authorities. In the United States, compulsory spraying is rapidly becoming the rule, and the standardisation of spraying fluids is its natural consequence. This standardisation merely considers the percentage of the *toxic* agent, and entirely disregards the equally important point of the wetting power.

A simple laboratory test for the determination of the relative wetting powers of different preparations is in urgent demand, and various methods of comparison have been suggested from time to time. Generally speaking, these suggestions have not been based upon an exact knowledge of the principles underlying the process of wetting, and, in consequence, the results obtained by the use of such methods are of little value.

The generally accepted test is the determination of the surface tension of the preparation. Thus, for example, Brünnich and Smith (1914, p. 83) state that 'the wetting power of any liquid, or its property to form a uniform film upon a greasy surface, depends primarily upon its surface tension,' and they attempt to compare the wetting power of various liquids by measuring their surface tension by means of a stalagmometer. Vermorel and Dantony (1910, pp. 1144–5) distinguish between the *static* and *dynamic* surface tensions of soap solutions, and claim that the value of the static tension, as opposed to the true (or dynamic), affords a satisfactory criterion of the wetting power. They point out that two solutions of sodium oleate of concentration 5 % and 0·1 % respectively, give the same capillary rise and wet equally well; nevertheless their true surface tensions differ very considerably. In a later paper, however (1912, pp. 1300–1), they state that 'the surface tension of a liquid is not sufficient to determine its wetting power....

The wetting power manifests itself very differently, according to the nature or the state of the substance to be wetted.... With regard to plants, the wetting power of some solutions appears to depend less upon the surface tension than upon the surface viscosity, as defined by Plateau.'

Lefroy (1915, pp. 291–3), in a paper dealing with the precise mode of action of *contact* poisons in insecticides, emphasises the importance of the wetting power of a spray-wash, and also touches upon the theory of wetting. He points out, that the capacity of a liquid to wet a solid surface is determined by the relative values of three distinct tensions: *viz.*, those of the Wash/Solid, Wash/Air and Air/Solid. He states that the condition for the wash to wet the solid is that the tension Wash/Solid must be less than the sum of the other two, *viz.*, Wash/Air and Air/Solid[1]. He points out that, as the tension Air/Solid remains constant, and the tension Wash/Solid is indeterminate, the tension Wash/Air is the only one to be considered, and the lower this tension is, so the more readily will the wash wet. Thus Lefroy's method of comparing the wetting power is nothing more than a determination of the surface tensions of the respective washes.

Other methods of comparing the wetting power of solutions have been based upon a comparison of the amount of liquid which adheres to a solid object of standard shape and size, which is dipped into the liquid and then carefully withdrawn. Naturally, the nature of the solid employed for the purpose is of considerable importance, and of those, the use of which has been suggested, mention may be made of the elytra of *Blatta Periplaneta americana*, strips of celluloid and of grease paper.

Most of the cattle dips in actual use contain a certain proportion of emulsified oil. It has been claimed that the presence of this emulsified oil increases the wetting power of the dip, and further, that the finer the state of this emulsion, the more marked is this effect. It has been urged, that the wetting power of such emulsified dips may be measured by a comparison of the fineness of the emulsified oil particles, and attempts have been made to compare the latter by observing the relative capacity of the dips to give a permanent foam.

A knowledge of the percentage of soap in any preparation gives no indication whatever as to its wetting power, so that the latter cannot be determined by means of a chemical analysis. Vermorel and Dantony (1911, pp. 972–4) have shown that the actual mode of preparation of a

[1] See footnote, p. 228.

fungicide has a marked influence upon its subsequent wetting power. For instance, two liquors, identical in their content of copper sulphate and sodium carbonate, but *prepared under diverse conditions*, required in the one case eight kilos of soap, in the other case only two kilos of soap to produce an equal wetting power.

Before we are in a position to appraise at their real value, these methods for determining the wetting power of liquids, it will be necessary to consider in some detail the various factors upon which the process of wetting depends. Though these factors were described in the main by Quincke (1877, p. 149) at an early date, and have since been referred to in various textbooks, yet they are so little understood by the ordinary chemist or biologist, that one may be excused for reiterating them here.

A drop of rain, falling freely through the air, assumes a spherical form, and behaves as if it were covered with a tightly stretched elastic film. The spherical form is the result of the pressure exerted by this surface film, and the film pressure itself is usually termed *surface tension*.

Now consider the case of a drop of oil, suspended in dilute alcohol of the same density as the oil (Plateau's experiment). The drop of oil also assumes a spherical form, as if bounded by a tightly stretching film. Obviously, the two cases are perfectly analogous, and the shape of the drop of oil is the result of a tension exerted at the interface of the oil and dilute alcohol. This tension is the surface tension of the oil to the dilute alcohol, but for the sake of distinction is usually known as the *interfacial tension*. Such a tension will exist at the interface of any two immiscible liquids, though in some cases it is known to be extremely small.

Consider now the surface of a solid. The idea of a stretched film exerting a distinct tension at the surface of a solid is not so readily conceived as in the case of a liquid. Nor can the existence of a solid surface tension be easily demonstrated experimentally. Nevertheless certain theoretical considerations place it beyond doubt that such a tension does exist at the surface of a solid[1].

Further, in the case of a liquid in contact with a solid, judging from analogy, we should expect an interfacial tension to exist between the liquid and the solid; and this is known actually to be the case.

[1] Surface tension may be regarded as surface energy per unit area, and it follows therefore that part of the energy of a solid body may be regarded as proportional to its surface, and that in this sense the body has a surface tension, this tension being measured by the energy per unit area of the surface.

We are now in a better position to understand the main factors upon which the wetting of a solid surface depends. Let us imagine that a drop of liquid has been placed upon the surface of a solid. Before we can decide whether it will retain its form, or whether it will spread out and cover the surface as a continuous film, *i.e.* wet the surface, consideration must be given to the relative values of three distinct tensions[1].

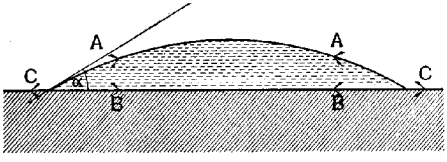


Fig. 1.

There is the surface tension of the liquid, which we will designate T_1; the surface tension of the solid T_2; and the interfacial tension of the liquid to the solid T_{12}. The surface tension of the liquid T_1 will be exerted in the direction of the arrows at A (Fig. 1), and will tend to maintain the spherical form of the drop. Similarly, the interfacial tension T_{12} will be exerted in the direction of the arrows at B, and this tension will also tend to roll up and maintain the drop in its spherical form. The surface tension T_2, however, will have the opposite effect; it will be exerted in the direction of the arrows at C, and will strive to draw out the drop into a more and more lenticular form, and the angle of contact (α) of the drop becomes smaller and smaller, until finally the latter forms a continuous film over the surface of the solid. For this to happen, it is not difficult to see that the surface tension of the solid T_2 must exceed the sum of the surface tension of the liquid T_1 and the interfacial tension T_{12},

i.e. $$T_2 > T_1 + T_{12}.$$

Since two of these tensions, *viz.*, the surface tension of the solid T_2, and the interfacial tension T_{12}, cannot be determined experimentally, it is impossible to apply this equation to a simple practical test. Röntgen (1878, pp. 324–8), however, has succeeded, by indirect means, in demonstrating its validity for the system Rubber/Water.

From the above equation, it is evident that for a liquid to possess a high wetting power, it shall have a low surface tension (T_1) and also

[1] If the drop of liquid is sufficiently small, the effect of gravity need not be considered.

a low interfacial tension (T_{12}). Further, it is evident from the consideration of Fig. 1, that the effect of reducing the interfacial tension T_{12} will be to cause the liquid to spread over the surface of the solid, even if the surface tension T_1 is fairly high; or in other words, it is the interfacial tension T_{12}, rather than the surface tension of the liquid T_1, which is the determining factor in wetting power.

The process of wetting, however, is not so simple as would appear from a consideration of the above equation. There are at least two other factors which must be taken into account, which, under certain conditions, may cause the above relationship to fail entirely.

The first of these disturbing factors is concerned with the solvent properties of the wetting liquid. It almost always happens in actual practice that the surface to be wetted is already coated with a layer of some greasy or waxy secretion. Since wetting depends partly upon the interfacial tension of the liquid to the solid, actual contact between the two is essential. It is necessary therefore that the wetting liquid should have a certain solvent action on the grease or wax, with which the solid may be coated. This solvent power need not be very pronounced, provided that the interfacial tension of the liquid is small, for, provided the surface of the solid is reached in any one place, the greasy or waxy layer will be displaced by the wetting liquid, because its interfacial tension to the solid will be lower than that of the grease or wax to the solid. If, on the other hand, the grease or wax is completely insoluble in the liquid, there can be no actual contact between the solid and the liquid, and therefore no wetting. Thus, for example, it is probable that the reason why mercury fails to wet the surface of so many solids is not entirely due to its high surface tension, but to the fact that it is incapable of dissolving the surface layer of moist air which adheres to the surfaces of most solids (Freundlich, 1909, p. 176).

The second factor, liable to vitiate conclusions drawn from the equation conditioning wetting, is concerned with the phenomenon known as *surface concentration* (capillary adsorption). If, with increasing concentration, an aqueous solution of a substance decreases the surface tension, it will be found that the solute tends to aggregate in the surface layers of the solution (Milner, 1907, p. 96). Saponin, proteins and various other substances possess this property to a marked degree. Thus, for example, Lewis (1908, p. 513) has calculated that with a 0·25 % solution of sodium glycocholate, there is an extremely thin surface layer, the degree of concentration of which is about 160 times that of the bulk. The result of this surface concentration is the

peculiar superficial viscosity or rigidity, first observed by Plateau (1873, pp. 261–96). In the case of saponin, this surface viscosity or rigidity may be readily demonstrated. If a bubble is blown with a 1 % solution of saponin, the bubble film will be found to be imperfectly elastic, and quite different from a soap film. If air is withdrawn while the orifice of the pipe is held in a vertical plane, so as to disturb the symmetry of the bubble, the shrinking film assumes a crinkled appearance, until finally there results a vertical semi-solid cylinder with almost opaque walls.

Vermorel and Dantony (1912, pp. 1300–1) were the first to point out that a liquid, exhibiting surface viscosity to a marked degree, had in consequence a high wetting power. Solutions of saponin have marked wetting properties, although the surface tension of such solutions is comparatively high. Thus, a 1 % solution of saponin is capable of wetting a glass plate, coated with paraffin wax, although a 5 % solution of soap fails to do so. The wetting power of solutions of saponin, gelatine, etc., seems to depend largely upon their capacity to form liquid planes, the high superficial viscosity of which prevents rupture and running together to form drops.

The phenomenon of surface concentration is, however, of interest to us from another point of view. It has long been observed that the apparent surface tension (static) of soap solutions is practically identical for all concentrations between 10 % and 0·1 % (Marangoni, 1871, p. 342, footnote). More recent investigations have, however, shown that this statement is only true, if the surface tension determination has been carried out on an *old* surface. Rayleigh (1890, p. 285) succeeded in showing that, if the surface tension of a 0·25 % solution of sodium oleate is measured within 1/400 of a second after the formation of the surface, the surface tension (dynamic) approximates closely to that of water. This initial tension, however, quickly falls until the ordinary value for the surface tension is reached. Rayleigh's results have been repeatedly confirmed by other workers, and a distinction is now drawn between what are known as the *static* and *dynamic* surface tensions. The static tension is the value obtained, when the determination is carried out on an *old* surface, as with the capillary rise and stalagmometer methods. The term 'surface tension' as generally used in textbooks refers to *static* surface tension. The *dynamic* surface tension on the other hand is the value obtained with determinations on perfectly fresh surfaces, as in the methods of surface tension determination by means of jets, surface ripples, etc. The *dynamic* tension is always ill-

defined; its value is dependent upon the age of the surface, and gradually passes into that of the *static* tension. The explanation of this peculiar behaviour of soap and other solutions is to be found in the property of surface concentration. Since the presence of soap reduces the surface tension, soap will become concentrated in the surface layer. As the surface tension is a function of the surface layer only, the presence of this concentrated surface pellicle of soap solution will be evidenced in a low surface tension. The peculiar behaviour of soap referred to above, would therefore find a ready explanation in the production of a surface pellicle of soap, the concentration of which is but little affected by that of the bulk of the solution. The formation of this surface pellicle is not instantaneous, but requires a certain amount of time, and it is upon this fact, that the existence of static and dynamic tensions, with the gradual passage of the one into the other, is due.

We are now in a better position to consider the various methods, which have been suggested from time to time for the measurement of wetting power, and to see how far these are suited to the purpose.

Firstly, can the surface tension (static) of a liquid, containing soap, be accepted as a reliable guide to its wetting power? We think not, for two good reasons. As we have seen, the conditions for a liquid to wet a solid surface are expressed in the equation:

$$T_2 > T_1 + T_{12}.$$

Since wetting is thus dependent upon three distinct tensions, it is obviously unwise to draw any conclusion as to its value, based on the surface tension of the wetting liquid T_1 only, even if the other two tensions are indeterminate. Vermorel and Dantony (1912, p. 1300 et seq.) have proved experimentally that solutions of different surface tensions may have the same wetting power. Thus, a solution of sodium oleate at 2·5 parts to 10,000 parts does not wet 'feuilles de Gamay,' but wets sulphur very well. The wetting power thus manifests itself very differently according to the nature or state of the substance to be wetted. The frequently recurring statement, that liquids with a low surface tension wet more readily than those with a higher one is not strictly true, because it may happen that a liquid with a high surface tension and low interfacial tension will wet better than one with a low surface tension and a high interfacial tension. In other words, the interfacial tension has a greater determining value than the surface tension, and for this reason, if it is desirable to determine one tension only, the interfacial affords a much more reliable guide to the wetting power than does the surface tension.

The second reason why we do not consider the surface tension to be a suitable criterion of the wetting power of a solution is the fact that, whilst, within limits, the wetting power increases with the concentration of soap, the surface tension (static) of soap solutions is practically independent of the concentration. As we have already seen, this independence is due to surface concentration. It is, of course, open to argument that the surface viscosity which results from this surface concentration, and which itself greatly assists wetting, may serve to counteract the lack of soap in the bulk of the liquid. Although not actually stated, this is possibly the idea underlying the claim of Vermorel and Dantony (1910, p. 1144 et seq.), that very dilute solutions of sodium oleate wet solid surfaces just as well as concentrated solutions, and that, whilst the true tension, by which they evidently mean the *dynamic*, is of no practical concern in regard to wetting power, the static surface tension as determined by capillary rise or the stalagmometer affords a practical criterion of wetting power. We shall show later (p. 233), however, that up to a concentration of about 3 %, the wetting power of a soap solution does actually increase progressively with the concentration. For the present, a simple experiment will serve to illustrate the point. Solutions, containing respectively 10 %, 1 % and 0·1 % of oleic soap all possess practically the same surface tension. If their wetting power be tested by their capacity to form a continuous film on a piece of grease paper, it will be found that the 10 % and 1 % solutions give a positive result, whilst the 0·1 % solution entirely fails to wet. In passing, it may be observed that the power to form a continuous film does not necessarily indicate that the 10 % and the 1 % solutions have the *same* wetting power. It is evident from the diagram (Fig. 1, p. 223) that when complete wetting occurs, the angle of contact between the solid and liquid is zero. When benzene is poured on to a clean glass surface, a continuous film of liquid is produced, or in other words, the benzene wets the glass. If ether is substituted for benzene, a continuous film is also produced, and we might therefore be inclined to assume that the wetting power of ether is equal to that of benzene. This however is known not to be the case. The angle of contact between benzene and glass is zero, which indicates complete wetting. The angle of contact between ether and glass is 16°, which is evidence that wetting is not complete. The wetting powers of benzene and ether are therefore not identical, although both give a continuous film on glass.

Sufficient evidence has now been brought forward to indicate

definitely that a determination of the static surface tension is insufficient for the purpose of an estimate of the wetting power of a solution containing soap, and this therefore disposes of all the methods as suggested by Brünnich and Smith, Vermorel and Dantony, and Lefroy[1], of comparison of wetting powers by such determinations.

We now come to the question as to how far the weight of liquid, adhering to a strip of a not easily wetted solid, which is immersed in the liquid and then carefully withdrawn, can furnish a criterion of the wetting power of the liquid in question. We have already seen that the power to wet is dependent upon one or both of two main factors: (1) a low surface tension and interfacial tension, and (2) a high surface viscosity. One of the results of a low surface tension and interfacial tension, where surface concentration effects are not very marked, is to cause the liquid to 'run' easily. With tree sprays, this is a most desirable property, in order that the liquid may penetrate the folds and interstices of the leaves etc. In the test of wetting power under consideration, the effect of a low surface tension and interfacial tension would be to wet the surface of the immersed solid completely, from which, on withdrawal from the fluid, all excess of liquid would 'run' off very rapidly, but still a continuous liquid film would remain adhering to the surface. The effect of a high surface viscosity would be in the opposite direction; as soon as ever a thin film of liquid was produced, the surface viscosity would come into play, as a result of which draining would be largely hindered. The amount of liquid adhering to the solid after immersion would therefore be the resultant of two opposite effects, and as such, we should not expect it to differ in any marked degree, whatever the liquid under investigation. Our experiments have proved this actually to be the case. The solid used for immersion experiments was a piece of wide glass tubing, coated internally and externally with a thin layer of collodion. Preliminary trials showed this to be the most suitable surface. Three liquids were used for the purpose of comparison: distilled water, a 1 % saponin solution, and a castor soap solution containing emulsified *green oil*[2]. The liquid under investigation, contained in a large weighing bottle, in order to prevent loss through evaporation, was weighed both before and after

[1] It may be desirable to point out that Lefroy, after appreciating the fact that the process of wetting is dependent on the relative magnitude of three surface tensions, *viz.*: Wash/Solid, Wash/Air and Air/Solid, has confused their relationship. The condition for wetting is, of course, that the Air/Solid tension should exceed the sum of the other two.

[2] A neutral oil of high boiling point obtained in the distillation of coal-tar.

the immersion of the solid; the difference in the two weights represented the amount of liquid removed on the surface of the solid. Two experiments with distilled water gave 0·0282 gm. and 0·0356 gm. respectively, as the weight of adhering liquid. Since water does not wet a collodion surface, theoretically no water should be removed by the solid. In actual practice, a variable quantity of water is removed in the form of adherent droplets, and this fact alone would seriously limit the possibilities of the test. With the 1 % saponin solution, which possesses the property of surface concentration to a marked degree, the weight of adhering liquid was 0·0764 gm. With the green oil emulsion, a liquid which had a low surface tension and interfacial tension, and 'ran' well, the corresponding weight was 0·0520 gm. Considering the very great difference in wetting power of the three liquids used, the difference in weight of the adhering liquid is very small, exactly as theoretical considerations had led us to surmise; such differences in weight are altogether too small to serve as criteria of wetting power.

Lastly, there is the question of *foaming* power. It has been rightly claimed that a solution containing a small quantity of oil in a finely emulsified state, possesses a high wetting power, and it will be seen later why this is so. Does the power of forming a permanent foam afford any indication of the fineness of the emulsion and therefore of the wetting power of the liquid? If, for a moment, we consider those properties upon which foaming and emulsification depend, we shall see that they are but indirectly connected.

Donnan (1899, pp. 43–9) has shown that a soap solution acts as an emulsifier by virtue of its low interfacial tension with respect to the oil. The result of this low interfacial tension is twofold: firstly, the soap becomes concentrated at the surface of the globules of oil, and owing to the surface viscosity produced thereby, the film of liquid intervening between two oil globules resists thinning and the consequent coalescence of the globules. Secondly, the force tending to break the intervening film is that of the interfacial tension, and this being low, is not strong enough to withdraw the aqueous film separating the globules. Hillyer (1903, pp. 516–21), working independently, has arrived at similar conclusions as to the cause of the permanence of emulsions. Stable emulsions of oil in soap solution are therefore dependent upon a low interfacial tension between the oil and the aqueous liquid, and the surface tension of the latter plays no part in the process.

The property of foaming and the factors which lead to the formation of a permanent foam, have formed the subject of a considerable amount

of investigation by various workers, amongst whom may be mentioned: Plateau (1873, pp. 1–119), Quincke (1888, pp. 580 et seq.), Rayleigh (1890 *a*, pp. 85 et seq.), Donnan (1899, p. 49), Shorter (1912, pp. 629–32; 1914, pp. 718–20). The generally accepted view of the nature of a foam is that it is an emulsion of air in a liquid. Support is lent to this view by the fact that the requisite conditions for the production of a *permanent* foam are exactly analogous to those necessary for the formation of a permanent emulsion of oil in an aqueous liquid. These we have seen to consist in (1) a high surface viscosity of the intervening film, and (2) an interfacial tension so low as to be incapable of breaking this film. In the case of an oil emulsion, the intervening matrix prevents the coalescence of the oil globules, and similarly, in the case of a foam, it is the intervening matrix which prevents the coalescence of the minute bubbles of air. With emulsions of oil, therefore, it is the interfacial tension of the aqueous liquid to the oil, which is the determining factor; with foams on the other hand, it is the interfacial tension of the aqueous liquid with air, *i.e.* the surface tension of the aqueous liquid, which is of importance. The property of giving a lasting foam therefore indicates that the liquid possesses (1) the property of surface concentration, such as saponin solution, solutions of which show quite exceptional frothing properties; and (2) a low *surface* tension, such as soap solutions. Pure liquids of low surface tension, as benzene, ether, do not give permanent foams. This indicates that surface concentration plays an essential part in the production of foams. It is now evident that, since a fine oil emulsion is dependent upon a low *interfacial* tension between the oil and aqueous liquid, and since foaming is dependent upon a low *surface* tension of the liquid, the power to give a lasting foam gives no indication of the emulsifying properties of the liquid towards oil, and therefore of the state of the oil emulsion contained in the liquid. In other words, foaming power is in no way indicative of a high wetting power. As a matter of fact, one emulsion which we examined, was found to have an extremely low interfacial tension, but the foaming power of the liquid was practically nil.

From the criticism which has been made of the methods hitherto suggested for the comparison of the wetting power of various solutions, it will be evident that the determination of wetting power is fraught with no small difficulty. And so far as liquids, which contain saponin, gelatine and other substances exhibiting surface concentration to a marked degree, are concerned, we do not know of any method by which these difficulties may be overcome. In the case of fluids made

up with a soap basis, however, which is the case with most cattle dipping fluids and horticultural sprays, we have worked out a method which affords a convenient and ready means of comparing their wetting powers. The method is a slight modification of that employed by Donnan (1899) for the determination of the emulsifying action of soap solutions. We realise that the method is open to certain objections, and that the results obtained cannot be regarded as absolute. At the same time, we venture to think that it is based on sound theoretical principles, and that the results obtained by its employment are sufficiently accurate for all *practical* purposes.

If we consider once more the equation:

$$T_2 > T_1 + T_{12},$$

it is evident that, if these three tensions could be determined, the expression $T_2 - (T_1 + T_{12})$ would represent the wetting power, provided that the disturbing factors of solubility and surface concentration did not come into play. As already stated, the surface tension of a solid and the interfacial tension of a solid to a liquid are indeterminate, from the point of view of a practical test. The interfacial tension between a thick oil and an aqueous liquid can, however, be determined without any great difficulty; and, as the surfaces to be wetted by dips or sprays are usually of a greasy or a waxy nature, there does not appear to be any valid reason why in the comparison of wetting power, a thick oil should not be taken to represent the solid surface.

In our method for the determination of the wetting power of a dip or spray fluid, a thick paraffin oil (liquid vaseline), free from acid, was taken as a standard to represent the greasy surface (*e.g.* a greasy hide, a waxy leaf). The surface tension of this oil was determined once for all by any convenient method, *e.g.* capillary rise. To compare the wetting power of any two preparations (soap), it was then only necessary to determine their surface tension and their respective interfacial tension towards the standard oil, and to substitute these values in the above equation. The values obtained in an actual comparison of the wetting power of two cattle dips are tabulated below. The determinations were made on the liquids diluted with water to the concentration usually employed for dipping, and in this particular instance castor oil was employed as the standard. As the two dips under consideration were both practically neutral in reaction, the use of this oil was permissible, but as a general rule, a neutral mineral oil is preferable.

The values in the last column of Table I indicate that Dip A should have the power of wetting a surface made greasy with castor oil, whilst Dip B should lack this power. This was found actually to be the case. A glass slide, smeared with a thin layer of castor oil, was completely covered and wetted when the dilution of Dip A was poured over it, whilst a similarly prepared slide, on being treated in the same manner with the dilution of Dip B, was found to assert its greasy nature, the dipping fluid running into drops and failing to wet[1].

TABLE I. *Wetting Power of Cattle Dipping Fluids towards Castor Oil.*

Dip	Surface tension of diluted dip T_1	Surface tension of oil T_2	Interfacial tension of oil and diluted dip T_{12}	$T_2-(T_1+T_{12})$ = wetting power
A.	34·6 dynes per cm.	38·1 dynes per cm.	0·4 dynes per cm.	+3·1 dynes per cm.
B.	40·8 dynes per cm.	38·1 dynes per cm.	10·0 dynes per cm.	−12·7 dynes per cm.

By methods to be described later, we have determined the surface tension, and the interfacial tension towards liquid vaseline, of solutions at various concentrations of most of the commercial soaps usually employed in the preparation of dips and sprays. There is no particular point in the publication of all these results. These various determinations were carried out to ensure that this method of comparing wetting power is applicable to all kinds of soap. As the results obtained with the different soaps did not vary markedly from one another, those given by sodium oleate are tabulated below and may be taken as typical. As commercial soaps vary so greatly in composition, the concentration of soap is expressed as percentage of fatty acid. The determinations were all carried out at a uniform temperature of 20° C.

Owing to the low surface tension of the liquid vaseline employed, the values obtained for the expression $T_2-(T_1+T_{12})$, *i.e.* the wetting power, are all negative. This indicates that the soap solutions used are incapable of wetting the vaseline, which actual trial with a slide

[1] Of course, in time both dips would wet the *glass* owing to the displacement of the oil from the glass surface. What we are at present interested in is the power of the dips to wet the *surface* of the oil.

smeared with vaseline showed to be the case. The fact that the vaseline is not wetted by soap solution, however, in no way precludes its use as a standard oil in the determination of wetting power.

TABLE II. *Wetting Power of Sodium oleate Solution towards liquid Vaseline (at 20° C.).*

Concentration	No. of drops	Surface tension of soap solution T_1	Surface tension of oil T_2	Interfacial tension of oil and soap solution T_{12}	$T_2-(T_1+T_{12})$ = wetting power
		dynes/cm.	dynes/cm.	dynes/cm.	dynes/cm.
2·0 % F.A.	—*	31·00	31·11	1·2	−1·2
1·0	1300	29·35	31·11	5·87	−4·11
0·5	1134	28·61	31·11	6·73	−4·23
0·25	730	28·28	31·11	10·46	−7·63
0·10	453	28·78	31·11	16·85	−14·52
0·05	274	31·34	31·11	27·86	−28·09
0·01	141	33·75	31·11	54·14	−56·78
0·001	119	55·00	31·11	61·14	−85·03
0·0001	117	70·99	31·11	65·24	−105·12

* This concentration 'streamed' in our usual drop-pipette. The interfacial tension was determined therefore in a pipette with a finer orifice. The number of drops is not given, as it is not comparable with the other figures.

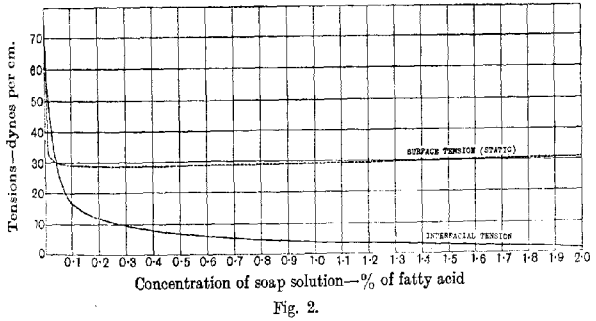


Fig. 2.

The values for the surface and interfacial tensions are plotted in Fig. 2; the tensions as abscissae, the concentrations of fatty acid as ordinates. The forms of the two curves are markedly different. With concentrations of more than about 0·1 % of fatty acid, the surface tension is only very slightly affected by rising concentration, the

tendency being to increase rather than decrease; the interfacial tension, on the other hand, decreases progressively. This difference is evidently to be explained by surface concentration phenomena, which are not nearly so marked at the interface of the oil and soap solution as at that of the soap solution and air. We have carried out exactly similar determinations on solutions of castor oil, sulphonated castor oil, linseed oil, resin and commercial soft soaps, and in every case we have found that, whilst the surface tension, at concentrations higher than 0·1 % of fatty acid, has shown but little variation with the concentration, the interfacial tension has decreased progressively up to about 2 % of fatty acid, when the interfacial tension usually approaches zero.

It is for this reason that in the actual practical method of comparison of the wetting power of dips and sprays, we do not recommend the determination of the three tensions T_1, T_2 and T_{12}.

It is obvious from the results given in Table II and illustrated graphically in Fig. 2, that of these three tensions, it is the interfacial tension (T_{12}) which exerts the predominating influence, so far as wetting is concerned. It is for this reason, coupled with the fact that the interfacial tension is the least affected of any by surface concentration effects, that we consider that the interfacial tension forms a better criterion of the wetting power than the expression $T_2 - (T_1 + T_{12})$, which, since it contains T_1, is itself vitiated by such concentration effects.

The method which we have employed for the determination of surface tension of the soap solutions (see Table II) is Serle's Torsion Balance Method (1913) which proved very convenient for the purpose. It is rapid and fairly accurate, and, where a large number of comparative determinations have to be made, can be confidently recommended. As, however, a determination of the surface tension is not necessary in the method of comparison of wetting power which we advocate, it is beyond the scope of this paper, and the reader is referred to the original communication.

Our method of estimating wetting power merely resolves itself into a determination of the interfacial tension of a standard thick paraffin oil with the liquid under investigation. There are several methods by which this interfacial tension may be measured, but the one most usually adopted now is Donnan's (1899) drop-pipette method: and, after some preliminary trials with most of the other methods, it quickly became evident that Donnan's method was the only one suitable for our purpose.

In this method, the oil is run from a pipette through the aqueous liquid, and the number of drops formed from a definite volume of oil are counted. The size of the drop and hence the drop number is

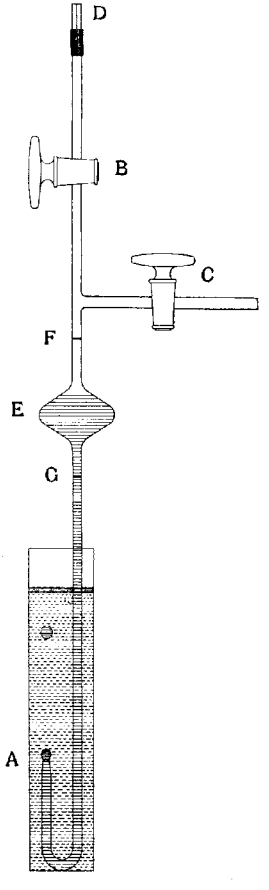


Fig. 3.

dependent upon (1) the interfacial tension, (2) the size of the aperture of the pipette, (3) the difference in density between the oil and the aqueous liquid, and (4) a constant depending upon the size of the

aperture, the difference in density and the interfacial tension. Where it is desired to state the interfacial tension in absolute terms, all these four factors must be taken into account. The calculations involved are somewhat tedious; they have been dealt with fully by Lewis (1908), to whose paper those interested are referred. The values given in Table II for the interfacial tension were obtained by this method.

Where comparative values only are required, as is the case in the determination of wetting power, the interfacial tension may be taken as inversely proportional to the number of drops, since the diameter of the aperture remains constant, and the difference in density between the oil and the aqueous liquids does not usually vary greatly, owing to the high dilution at which the dip or spray fluid is employed. As we have already seen that the wetting power varies indirectly as the interfacial tension, it follows that the wetting power is directly proportional to the drop number.

The form of pipette used for the determination of the interfacial tension is shown in Fig. 3, which requires little explanation. The lower limb (*A*) is curved round, and its aperture is somewhat restricted and ground carefully to a plane surface. The walls of the aperture should be fairly thin, in order to prevent unevenness in wetting. Both the upper limb of the pipette, as well as the lateral tube with which it is provided, are closed with glass stop cocks (*B* and *C*). The lateral tube is used for filling the pipette by suction. The upper limb terminates in a very fine capillary tube (*D*), which is best attached by means of rubber tubing. The object of this capillary is to regulate the flow of oil through the aqueous liquid, and should be so selected as to allow an interval of about ten seconds between successive drops, when water is used.

The volume of the pipette used in our experiments was 49·1 c.c., the length from the bottom of the bulb (*E*) to that of the bend 22·5 c.c.; the diameter of the aperture was 3·5 mm. Later experience has indicated that a pipette with a capacity of 25 c.c. and an aperture 2·7 mm. in diameter would probably be found most convenient for general purposes. The paraffin oil used as a standard was a liquid vaseline, which was almost acid-free and possessed a density of 0·8690[1].

An actual determination is carried out as follows. The preparation (*e.g.* dipping fluid) is diluted[2] to the strength at which it is generally

[1] Obtained from the Chesebrough Manufacturing Co., Holborn Viaduct.

[2] In order to obtain comparable results, the dipping fluid should always be diluted immediately before the count is made.

used in practice, and 250 c.c. of the diluted liquid are placed in a gas jar (20 cm. high × 5·5 cm. internal diameter). The pipette is then filled to above the upper mark (*F*) with oil, and, after carefully wiping the outside of the pipette and the surface of the orifice, the pipette is lowered into the gas jar and allowed to rest on its bottom. The pipette is maintained in a vertical position by means of a clamp. The top stopcock is then opened and the level of the oil allowed to sink to the *upper* mark. Any oil adhering to the orifice is carefully removed by means of a glass rod, after which the stopcock is again opened and the number of drops formed whilst the level of the oil in the pipette sinks to the *lower* mark (*G*) are counted.

It may happen, with liquids of very low interfacial tension, that the oil flows from the orifice in a continuous stream instead of forming drops. This occurs when the interfacial tension is so low that the diameter of the drops is less than that of the orifice of the pipette, and with our pipette, it signified an interfacial tension of less than 0·66 dynes/cm. If streaming should occur, a pipette with a smaller aperture may be employed or the aqueous liquid must be further diluted.

It is advisable to keep a good stock of standard oil so that all determinations may be made on the same sample of oil and therefore be strictly comparable. The same lot of oil should not be used for more than one determination. As different samples of liquid vaseline differ slightly and give somewhat different drop numbers with distilled water, it is advisable to take the latter as a standard liquid, and to express the wetting power of the solution as the ratio of the drop number of the solution to that of distilled water, multiplied by 100. The following table contains the results of determination of the wetting power of different cattle dips. The dips were all diluted to the same degree, and the drop number determined at a temperature of 20°.

TABLE III. *Comparison of the Wetting Power of Cattle Dips.*

Solution	Drop number	Wetting power = $\frac{\text{Drop no. solution} \times 100}{\text{Drop no. water}}$
Dip A	585	380
„ B	290	184
„ C	543	353
„ D	386	251
Distilled water	154	—

We do not claim that this means of estimating wetting power is free from every objection, nor can it be regarded as affording absolutely

accurate values. The interfacial tension existing between an oil and an aqueous liquid is itself a somewhat indefinite quantity, and varies with the age of the oil globule. At the same time, we feel that the method will furnish a means, sufficiently accurate for the purpose, of determining and expressing *in numerical values* the wetting power of a solution, and therefore will supply a long felt need. The apparatus required for the test is simple, and at the same time there is no call for great manipulative skill in carrying out the determination. If attention is given to the few simple precautions mentioned, comparable and reliable results are easily obtained. It may be desirable to mention once again that the method is only applicable to preparations containing *soap* as a basis; with other bases, *e.g.* gelatine, saponin, etc., it entirely fails. With these substances, surface concentration at the interface of the oil and aqueous solution becomes so marked that quasi-solid surfaces are produced, which completely vitiate the drop number and therefore the value for the interfacial tension. We are at present endeavouring to find a simple method by which the wetting power of solutions of such substances may be compared, but up to the present have been unable to evolve anything satisfactory.

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SOIL GASES.

By J. WALTER LEATHER, V.D., F.I.C.

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At page 5 of their recent communication on the composition of soil gases[1] Messrs Russell and Appleyard give some data regarding gas which was extracted from portions of soil which had been placed in flasks, the air pumped out and the soil then allowed to stand for one or more days. The authors state that "the total amount of gas given up is not great." As a matter of fact a little consideration shows that the volume of this gas was indeed comparatively very large.

This gas may be compared with that originally included in the soil mass. Judging from the data given on page 44, the "free air" varied from 8 to 18 c.c. per 61·8 c.c. or (61·8 × 2·31) = 143 grams soil, or from 5·6 c.c. to 12·6 c.c. per 100 grams dry soil. Reference to Table III shows that the gas which was obtained from the several portions of soil, subsequent to the removal of the air of the flasks, was considerably greater than the "free air" of the soil.

Considering secondly the composition of this gas, it is evident that its origin is certainly not accidental. The authors state that it consists "mainly of carbon dioxide with some nitrogen." Here again the figures hardly support such a definition, for the nitrogen exceeds the carbon dioxide in a number of the samples. Again, although to a certain extent the carbon dioxide could be assumed as having been derived from calcium bicarbonate solution in the soil, its quantity is very far greater than could be present in this form. The water present could only hold a certain amount of calcium bicarbonate in solution, and it is from this item that the above deduction can be made. Lastly the volume of nitrogen obtained is very far in excess of what could be assumed to be condensed or dissolved on soil particles.

[1] This *Journal*, Vol. VII. pp. 1–48.

I have subjected the Pusa soil to the same treatment as that employed by Russell and Appleyard. A portion of soil, which was subsequently found to include 382·8 grams dry soil and 32·8 grams water, was placed in a bottle and the air pumped out. It was then left for four days connected with the Töpler pump. At the end of the first 24 hours a few c.c. of gas were obtained, after a further 24 hours another small quantity was removed, and on the fourth morning a much less quantity was present. The total gas obtained measured 7·27 c.c. and included carbon dioxide 5·62 c.c., oxygen 0·10 c.c. The volume of gas originally present in this soil would be about 100 c.c.

In other two experiments the soil was taken in the special tool which I have devised for the examination of soil gases. In this case the gases are removed from the *undisturbed* soil. The volume of soil was 154·4 c.c. in each case. In the first experiment 61·19 c.c. of gas was removed at once. After standing for 48 hours 1·87 c.c. of gas was removed, of which 0·87 c.c. was carbon dioxide. In the second of these experiments the gas first removed was 52·33 c.c. After standing for 48 hours, 2·80 c.c. more gas was obtained, of which 2·18 c.c. was carbon dioxide. It is evident that this soil behaves differently from the Rothamsted soil. The undisturbed soil yields after 48 hours a small quantity of gas, most of which was probably present when the sample was taken; but the soil after disturbance yields a small amount of a gas mixture which is qualitatively similar to that obtained at Rothamsted, but is comparatively very small in quantity.

Soils will no doubt vary in respect of the amount of gases which their solid constituents can condense, but I may here mention that the soil of this part of India condenses so little that it is difficult to estimate it; it is certainly less than 4 % of the whole of the gases present in the soil. Details regarding this and other features of the soil gases present in the land of this Institute are about to appear as a memoir.

One is forced to the conclusion that the gas which was obtained on successive days by Russell and Appleyard was *formed* and liberated gradually, presumable by bacterial action.

PUSA,
9th June, 1915.

REPLY TO DR LEATHER.

In our paper we did not discuss Table III in any detail because as we stated the dissolved atmosphere in the soil is under further investigation. It is sufficient to say that the gas was not obtained in the manner suggested in the opening lines of the above note. The dissolved gas begins to come out directly the pressure is lowered by the working of the pump, and no sharp line can be drawn to indicate precisely what fraction of the extracted gas was free and what dissolved. For this reason we refrained from making any estimate of the amount of the dissolved atmosphere except to say that it is small. No comparison can be instituted between the data in Table III and those on page 44 owing to the difference in experimental conditions, and, moreover, the first and second lots of 30 c.c. recorded in Table III, on the composition of which Dr Leather bases part of his argument, certainly contained some of the free air. The successive extractions were not made on successive days: in some cases there had been practically no interval between them.

The conclusion we drew from the experiments was that the atmosphere dissolved in the surface films of water and other substances is almost devoid of oxygen and consists mainly of carbon dioxide with some nitrogen. This seems to us to be a legitimate deduction from the figures, and we do not think we can go any further at present. Dr Leather says that the Pusa soil behaves differently. The figures he quotes, however, seem to lead to the same conclusion: in his case also the amount of gas is small and it appears to be poor in oxygen. Where then is the difference?

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ON OVARIOTOMY IN SOWS; WITH OBSERVATIONS ON THE MAMMARY GLANDS AND THE INTERNAL GENITAL ORGANS.

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(School of Agriculture, Cambridge.)

Part IV.

In Part III[1] of this series of communications we recorded that no mammary pigment could be found in the old sows that were examined by us, and we suggested the possibility that such pigment had previously been present and had been destroyed or removed during the periods of glandular activity.

In the present note we record the results of four experiments which were undertaken to test this hypothesis.

Four sows of coloured varieties were taken, three being Large Blacks and one a Berkshire. Each in turn was placed upon an operating table and anaesthetised. Incisions were then made with a scalpel in the tissue of two of the mammary glands, each cut being sufficiently deep and extensive to reveal the existence of mammary pigment if such were present in the neighbourhood of the nipple. After the examination had been completed, the incised tissue was stitched up and dressed with cotton wool and collodion. The wounds healed by first intention. The four sows were all operated upon on the same day, October 15th, 1913.

The following are the details of the experiments together with the further history of each of the sows.

(1) In a Large Black sow two mammary glands were examined for pigment, namely those associated with the first (i.e., the most anterior) nipple on the right side, and the third nipple on the left side. Black pigment could readily be detected in each of the two glands. The sow

[1] This *Journal*, Vol. VI. May, 1914, p. 182.

subsequently had two litters, each of eight pigs, which were suckled. The first litter was born on March 29th, 1914, and the second on October 13th, 1914. On February 15th, 1915, the sow was killed, when all the mammary glands (including those previously examined) were closely searched for pigment. None whatever could be found.

(2) In a Large Black sow two glands were examined for pigment in the same manner as that adopted in the first experiment. The first gland on the right side did not appear to contain pigment, but in the fifth gland on the other side some was found a little distance from the nipple. The sow had a litter of nine on March 22nd and a litter of eleven on October 8th, 1914, and lactated normally. When the animal was killed on February 15th, 1915, not a trace of pigment was found in any of the glands which were carefully examined.

(3) In a Berkshire sow the right anterior gland was found to contain a large quantity of pigment, but none was found in the fourth gland on the left side. The sow on April 14th had a litter of six pigs and on October 13th a litter of five pigs which were suckled normally. On killing the sow on February 15th, 1915, no mammary pigment could be detected.

(4) In a Large Black sow the first right and third left glands were examined and each was seen to possess much pigment. The sow produced a litter of nine young on March 23rd, and a litter of eight on October 15th. Both were suckled. On killing the animal on February 15th, 1915, a very slight trace of dark pigment was found just below the first teat on the left side, but none in association with either of the other teats, so it was evident that pigment had been removed from the glands previously searched.

Each of the sows at the time of killing had large protruding follicles in the ovaries, indicating the approach of another heat period, and there was every indication that the animals were sexually normal.

The above described experiments prove very clearly that the dark pigment which so frequently occurs in the mammary tissue in pigs of coloured breeds, and which we have shown to exist even in the embryo, may be no longer present in sows which have been bred from. The precise period (or periods) at which this pigment disappears is still unknown, but there is a strong presumption that its removal takes place either during the progress of lactation, or in the period of pregnancy when the mammary glands are being built up preparatory to the secretion of milk. The fact that mammary pigment can become diminished in quantity to the point of disappearing altogether, is a point of great

importance to the investigator who might otherwise be in danger of concluding that certain individual sows belonged to a strain which did not carry mammary pigment, when in reality he would be dealing with cases in which such pigment had formerly been present in quantity and had subsequently been removed.

The operations were performed at the Field Laboratories, Milton Road, Cambridge, by F. H. A. Marshall.

The expenses of the investigation were defrayed by the Board of Agriculture and Fisheries out of funds allocated to them for purposes of research, by the Development Commission.

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THE EFFECT OF REMOVING THE SOLUBLE HUMUS FROM A SOIL ON ITS PRODUCTIVENESS.

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The decay of plant and animal residues in soils produces brown organic compounds commonly designated as humus. Some of it is soluble in dilute alkaline solutions, and agricultural chemists and investigators have often assumed that this part plays an important function in the nutrition of plants by reason of its solubility. As a result numerous methods have been devised for determining this readily soluble material, and its amount has been regarded as a measure of the fertility of the soil. Since however the amount of nitrogen in the soluble humus material is commonly 40 per cent. to 50 per cent. of the total nitrogen, the results for ordinary soils gave no better indication of potential fertility than did the total nitrogen determination itself[1].

Few experiments seem to have been made to ascertain directly if the soluble humus really does play any considerable part in plant nutrition. The only one that has been found is recorded by Grandeau[2] in 1872. A kilogramme of Russian Black Earth was divided into two parts; one was treated with dilute hydrochloric acid and ammonia solution to remove soluble humus, or, as he termed it, *matière noire*; the other was left untreated.

The two lots of soil were put into flower pots and watered to saturation point with distilled water, the untreated soil taking 7 per cent.

[1] "Results of Investigations on the Rothamsted Soils," 1902, by Bernard Dyer. N. H. J. Miller's "Determinations," p. 179.

[2] "Recherches sur le rôle des matières organiques du sol dans les phénomènes de la nutrition des végétaux," par M. L. Grandeau. *Publication de la Station agronomique de l'Est.* 1872.

more water than the other. Beans were planted in both: they developed normally on the untreated soil but failed entirely on the extracted soil.

During the past two seasons this experiment has been repeated on a larger scale, with results sufficiently interesting to justify their being placed on record. The removal of the soluble humus was effected by washing the soil with dilute hydrochloric acid to remove bases, and then repeatedly extracting with dilute soda. Some 40 per cent. of the nitrogen in the soils was thereby removed.

Vegetation experiments were then conducted with large quantities of untreated and extracted soils, and approximately equal total yields both of dry matter and of nitrogen were obtained over four successive crops. Thus it appears that the removal of the soluble humus had no effect in diminishing the productiveness of the soil in spite of the fact that the soil used was known to respond to nitrogenous fertilisers.

Laboratory experiments were then started to ascertain the effect of removing soluble humus on the rates of production of nitrate and ammonia in the soils. Here the indications were that the removal of the soluble humus increased the amount of ammonia but diminished that of nitrates in the soil, and the sum of ammonia and nitrate was usually less than in the untreated soil. The numbers of bacteria however were considerably increased. No marked difference was produced where 0·5 per cent. untreated soil was added to replace the bacterial flora that might have been destroyed by the acid and alkali treatment. These results are not necessarily inconsistent with the vegetation experiments. It has been shown in these laboratories that the accumulation of ammonia and nitrate in an uncropped soil will only proceed to a certain stage when it stops[1]. In vegetation experiments on the other hand the nitrate is perpetually being removed by the growing plant. There is thus a considerable difference in conditions.

Method of Extraction. 250 grams of air-dried soil, which has passed through a 3 mm. sieve, are shaken up with 2500 c.c. N/5 HCl. After standing in contact with the acid for one hour, the soil is thrown on to a filtering funnel and washed free of acid; 500 grams of soil treated in this manner are shaken with 2000 c.c. 2 per cent. NaOH and allowed to settle overnight. The dark-coloured humus extract is siphoned off. This process is repeated five times in all, and at the fifth extraction the liquid is of a lightish brown tint. The soil is then shaken with water, allowed to settle, the supernatant liquid is removed, and finally the

[1] "The Effect of Partial Sterilisation of Soil on the Production of Plant Food," by E. J. Russell, and H. B. Hutchinson. *Journal of Agricultural Science*, v. p. 193.

whole is made slightly acid with HCl to promote flocculation and allow of easy filtration and washing. The acid is then removed, the soil dried and passed through a 3 mm. sieve.

The soils used were: (1) Allotment soil, a medium garden soil; (2) Harpenden Field Soil (Arable), a typical loam. The treatment was found to reduce the nitrogen very considerably.

TABLE I.

Total Nitrogen in Untreated Allotment Soil, per cent.	·315
„ „ in Extracted „ „ „	·178
„ „ removed, per cent.	·137
Percentage reduction	43·6
Total Nitrogen in Untreated Field Soil, per cent. ...	·186
„ „ in Extracted „ „ „ ...	·109
„ „ removed, per cent.	·077
Percentage reduction	41·2

Vegetation experiments were made with the four soils (Untreated Allotment, Extracted Allotment, Untreated Field Soil, and Extracted Field Soil respectively).

Each pot had a capacity of 10 kilos.; 5 kilos. sharp sand were put at the bottom, and above this 5 kilos. of the soil under investigation, mixed with 15 per cent. of sharp sand. Each set consisted of 5 pots: thus there were 20 pots in all. Chalk was added to replace carbonates removed by the acid treatment and 1 gram KH_2PO_4 in solution was added to each pot. Thus the soils were as follows:

TABLE II.

	Untreated Allotment	Extracted Allotment	Untreated Harpenden	Extracted Harpenden
Fine Earth ...	5000 grams	5000 grams	5000 grams	5000 grams
Sand	750 „	750 „	750 „	750 „
Chalk	—	21 „	—	47·5 „
KH_2PO_4	1 gram	1 gram	1 gram	1 gram

Wheat was sown on March 14th, 1914, and the pots were kept in the glass-house till April 7th when the number of plants was reduced to five per pot and all the pots were removed to the open. The plants in all cases made slow growth; those in the untreated soils tillered better, but later in the growing period were caught up, and passed by those in the treated soils. Subsequent growth was quite satisfactory, and in August, when the crops were cut, the weights were:

TABLE III. *Weights of first crop, Wheat.*

Soil	Weight of Wheat dried at 100° C.	Percentage of Nitrogen in crop	Weight of Nitrogen in crop
	grams		grams
Allotment Soil	67·8	·95	·64
Allotment Soil Extracted	72·3	1·01	·73
Harpenden Field Soil	45·3	·82	·37
Harpenden Field Soil Extracted ...	67·2	1·06	·71

After the wheat was cut the soils were thrown out of the pots, mixed up afresh and replaced as before. Mustard was then sown (still in August), during rather dry weather, and was grown in the open. Only on Harpenden Field Soil was any appreciable yield obtained: on the extracted soil the plants scarcely grew beyond the seed-leaf stage. On both allotment soils the crops were poor. The mustard was pulled on October 8th and dried and weighed as before.

TABLE IV. *Weights of second crop, Mustard.*

Soil	Weight of Mustard dried at 100° C.	Percentage of Nitrogen in crop	Weight of Nitrogen in crop
	grams		grams
Allotment Soil	2·7	2·34	·06
Allotment Soil Extracted	3·9	2·51	·08
Harpenden Field Soil	13·1	1·93	·25
Harpenden Field Soil Extracted ...	·9	3·44	·03

TABLE V. *Weights of third crop, Rye.*

Soil	Weight of Rye dried at 100° C.	Percentage of Nitrogen in crop	Weight of Nitrogen in crop
	grams		grams
Allotment Soil	3·1	3·94	·12
Allotment Soil Extracted	3·7	3·68	·14
Harpenden Field Soil	5·4	3·66	·20
Harpenden Field Soil Extracted ...	6·6	4·17	·28

The soils were then cultivated but not otherwise disturbed. On October 13th all the pots were sown with rye, which grew in the glass-house till 18th February, 1915, when the plants, 10 to each pot, were

pulled, as it seemed unnecessary to allow them to ripen. The crops were dried, weighed, and the nitrogen determined as before. The soil in all the pots was thrown out and thoroughly dried before replacing in the pots and sowing with a fourth crop—mustard.

Mustard was sown in all the pots in April, 1915, and was left till the end of May when it was pulled, dried and weighed. There was a fair crop on all the soils except on the Extracted Harpenden Field Soil, where it failed entirely. The plants were cut when just flowering.

TABLE VI. *Weights of fourth crop, Mustard.*

Soil	Weight of Mustard dried at 100° C.	Percentage of Nitrogen in crop	Weight of Nitrogen in crop
	grams		grams
Allotment Soil	11·3	2·12	·24
Allotment Soil Extracted	10·8	2·19	·24
Harpenden Field Soil	15·4	1·97	·30
Harpenden Field Soil Extracted ...	1·3	3·53	·05

Table VII gives a summary of the crop and nitrogen data for the different soils.

TABLE VII. *Summary of weights of all four crops.*

Soil	Weight of Wheat	Nitrogen in Wheat	Weight of Mustard	Nitrogen in Mustard	Weight of Rye	Nitrogen in Rye	Weight of Mustard	Nitrogen in Mustard	Total Weight of Dry Matter produced	Total Weight of Nitrogen removed in four crops
	gms.	%	gms.	%	gms.	%	gms.	%	gms.	%
Allotment Soil	67·8	·95	2·7	2·34	3·1	3·94	11·3	2·12	84·9	1·07
Allotment Soil Extracted	72·3	1·01	3·9	2·51	3·7	3·68	10·8	2·19	90·7	1·19
Harpenden Field Soil	45·3	·82	13·1	1·93	5·4	3·66	15·4	1·97	79·2	1·13
Harpenden Field Soil Extracted	67·2	1·06	·9	3·44	6·6	4·17	1·3	3·53	76·0	1·07

Thus it will be seen that in five cases out of eight the plants obtained more nitrogen from the extracted than from the untreated soils, in one

case the amount was the same and in two cases the amount was less but on both these occasions the crop (mustard) failed on one soil.

TABLE VIII. *Nitrogen in soils and crops.*

Soil	Percentage of Nitrogen in Soil	Total Weight of Nitrogen per pot	Total Nitrogen removed in four crops
		grams	grams
Allotment Soil	·315	15·74	1·07
Allotment Soil Extracted	·178	8·88	1·19
Harpenden Field Soil	·186	9·28	1·13
Harpenden Field Soil Extracted ...	·109	5·46	1·07

During the winter of 1914–1915 additional quantities of the same two soils were extracted and used in vegetation experiments in precisely the same way as in the previous sets. In this instance the acid and alkali treatment removed a somewhat smaller amount of the soluble humus matter from both soils.

TABLE IX.

Total Nitrogen in Untreated Allotment Soil, per cent.	·326
„ „ in Extracted „ „ „	·206
„ „ removed, per cent.	·120
Percentage reduction	36·8
Total Nitrogen in Untreated Field Soil, per cent. ...	·177
„ „ in Extracted „ „ „ ...	·107
„ „ removed, per cent.	·070
Percentage reduction	39·5

In the case of the Allotment Soil, wheat was sown in the pots on November 12th, 1914. The plants in the untreated soil grew and tillered better than the others but in both sets the crops were good. They were cut when nearly in full ear on 23rd June, 1915.

Wheat was not sown in the Harpenden soils till 25th March, 1915, so that the growing period was much the same as for the wheat crops of the previous season. It was cut on 23rd June, 1915, when the ears were beginning to come out.

The yields were comparatively poor but during the whole time the plants in the extracted soil were greener in the leaves and did better than those in the untreated. The following figures were obtained for the four soils on cutting and drying:

TABLE X.

Soil	Weight of Wheat dried at 100° C.	Percentage of Nitrogen in crop	Weight of Nitrogen in crop
	grams		grams
Allotment Soil	148·9	·82	1·22
Allotment Soil Extracted	127·7	·82	1·05
Harpenden Field Soil	47·8	1·18	·56
Harpenden Field Soil Extracted ...	57·7	1·25	·72

Experiments on the rate of nitrate production in the soil. These were made with Untreated and Extracted Allotment Soils respectively, and they involved periodical determinations of nitrate, ammonia, and numbers of bacteria. Chalk was added to replace the carbonates removed during extraction, and a third set was put up consisting of extracted soil plus carbonate, seeded with 0·5 per cent. of untreated soil to replace the bacterial flora that might have been destroyed by the acid and alkali treatment. Estimations were made at the start, after one week, after three weeks, and after a considerable period. The soils were stored in bottles, and the moisture content adjusted to approximately 16 per cent., this being known as favourable for bacterial action. Table XI shows the changes that occur.

It will be seen that the formation of nitrate is less marked in the treated soils, and that ammonia tends to occur in quantity and to persist, whilst the untreated soil contains only the normal three or four parts per million. Further, while the numbers of bacteria in the untreated soil are normal throughout—about 20 millions per gram downwards—the numbers in the extracted soils are initially very low, then become abnormally high and remain above the usual level; there is however no corresponding increase in the production of ammonia and nitrate. This is very similar to the result obtained by W. Buddin[1] on treating soils with non-volatile antiseptics. Sowing the extracted soil with bacteria from untreated soil causes no marked difference in the results.

[1] *Journal of Agricultural Science*, 1914, VI. pp. 417–451.

TABLE XI. *Changes in bacterial numbers, in ammonia and nitrate, in soils before and after extraction of soluble humus.*

Time	Untreated Allotment				Extracted Allotment + $CaCO_3$				Extracted Allotment + $CaCO_3$ + ·5 % Seeding			
	Parts per million			Millions per gram	Parts per million			Millions per gram	Parts per million			Millions per gram
	Nitrate	Ammonia	Water %	Bacteria	Nitrate	Ammonia	Water %	Bacteria	Nitrate	Ammonia	Water %	Bacteria
					May 4th, 1914.							
Initial	6	2	16	12	8	7	2	—	8	8	2	—
After 1 week ...	15	—	16	2	9	7	14	34	10	9	14	31
After 3 weeks...	10	3	16	3	9	8	14	17	12	8	14	16
After 37 weeks	50	2	14	7	19	7	13	23	26	2	13	46
After 43 weeks	58	—	14	5	32	2	13	39	29	2	13	34
					November 26th, 1914.							
Initial	16	4	18	18	7	15	2	1				
After 1 week ...	25	3	18	28	18	18	16	119				
After 3 weeks...	39	3	18	10	21	15	16	167				
After 16 weeks	38	1	17	11	22	19	15	62				
					February 3rd, 1915.							
Initial	16	3	21	5	7	16	2	—	8	18	2	—
After 1 week ...	17	4	18	15	9	15	16	81	11	19	17	106
After 3 weeks...	24	1	21	17	13	15	17	63	12	17	18	107
After 14 weeks	39	2	21	11	13	17	17	48	30	14	16	40